ORIGINS OF CLERK MAXWELL'S ELECTRIC IDEA

SIR JOSEPH LARMOR

ORIGINS OF CLERK MAXWELL'S ELECTRIC IDEAS

LONDON
Cambridge University Press
FETTER LANE

NEW YORK · TORONTO
BOMBAY · CALCUTTA · MADRAS
Macmillan

TOKYO
Maruzen Company Ltd

ORIGINS OF CLERK MAXWELL'S ELECTRIC IDEAS

as described in familiar letters to

WILLIAM THOMSON

Edited by

SIR JOSEPH LARMOR

CAMBRIDGE
AT THE UNIVERSITY PRESS
1937

PUBLISHERS' NOTE

These letters from JAMES CLERK MAXWELL to his friend WILLIAM THOMSON were originally issued as Part V of Volume 32 of the Proceedings of the Cambridge Philosophical Society. As it is believed that they may interest a larger circle, they are now made available in their present form by arrangement with the Society.

March 1937

THE ORIGINS OF CLERK MAXWELL'S ELECTRIC IDEAS AS DESCRIBED IN FAMILIAR LETTERS TO W. THOMSON

JAMES CLERK MAXWELL, in his days of early development, made a practice of communicating his progress in ideas by informal letters to his scientific friends G. G. Stokes and W. Thomson, who were in the habit of preserving their correspondence. The record, so far as revealed in the letters to Stokes, has been published in volume 2 of Prof. Stokes' *Scientific Correspondence.* The letters which are here printed have emerged among Lord Kelvin's manuscript remains. They had been arranged apparently by Prof. S. P. Thompson when he was preparing his biography of Lord Kelvin's practical activities. I find that they had been examined by myself when a project of publishing Lord Kelvin's scientific correspondence was contemplated, after the manner of that of Stokes; which afterwards proved to be impracticable, as the material had largely been skimmed over.

The letters now published present a sharp and crisp account of the genesis and rapid progress of Clerk Maxwell's ideas as he groped towards a structural theory of the electric and magnetic field, here informally expounded by him in his close relationship to his friend Thomson—in fact it is an informal study in the natural mentality of a man of proved genius. Thomson's own early attempts in this domain will be found described in the extensive Obituary Notice published in the *Proceedings of the Royal Society* for 1906. The material is mainly in the reprint (1872) of his *Papers on Electrostatics and Magnetism* and in vol. I of his Scientific Papers. It will be noticed that Maxwell steers clear of submarine telegraphy, which in its long-distance domain had then the prominence that now attaches to the domain of wireless waves.

Thus be it noted that he already addressed his compatriot Thomson by his surname from 1854 onwards, whereas this familiar mode of personal approach in the case of Stokes dates only from 1857. (Maxwell became Bachelor of Arts at Cambridge in January 1854, Stokes in January 1841, W. Thomson in January 1845.)

An impressive representation of Maxwell's keenness in the early years is conveyed by a beardless photograph of about the time of the earlier letters, contributed by Sir J. J. Thomson to the centenary (1931) volume of essays. To this volume the present publication may be regarded as an Appendix. Scientific readers of the personal biography of Maxwell as drawn up by Prof. Lewis Campbell will welcome documents which illuminate the early evolution of his genius.

As the earliest formal essay towards development of Maxwell's electric views appeared in the *Transactions* of the Cambridge Philosophical Society, it seems fitting that this biographical record of evolution, in relation to Thomson, should find a place in the *Proceedings* of the Society. Thomson must have encouraged him, though we know that he maintained an attitude rather sceptical throughout his life as regards any claim of the theory to be more than provisional.

The autograph manuscripts of the letters will be preserved in the Collections of the Cambridge University Library, along with the main collection of letters to Lord Kelvin which contains autographs of most of the great Natural Philosophers of his time.

The earlier formal relations of the electric field here evolved were set out in the memoir "On Faraday's Lines of Force" (1856) (*Trans. Cambridge Phil. Soc.* **10**, 27–83), the later concrete developments into dynamical imagery, mainly of relations of magnetism in terms of vortices, as presented here (cf. p. 39), were set out later in the extensive memoir "On *Physical* Lines of Force" (*Phil. Mag.* 1861–1862) and largely incorporated in the final chapters of the *Treatise on Electricity and Magnetism.* The consolidating formal memoir "A Dynamical Theory of the Electromagnetic Field" (1864) appeared in *Phil. Trans.* **155** (1865), 459–512.

On the limitations of the modern statistical science, which Maxwell largely initiated, one may refer here to Maxwell's Essay on free will, etc. (1873) printed in the *Life*, p. 439. This paper treats of personal sense of freedom, as viewed in relation to dynamical instability and singular states of a physical system concerned therewith by analogy or otherwise. In the expert judgment of the late Prof. Harald Høffding of Copenhagen this essay merits higher philosophic rank than it received.

The problem of constructing a rational science of Thermodynamics out of the indications of genius provided by Carnot was distracting Thomson's mind in the years of the earlier letters, and this occupation is referred to conversationally in various places. As the earliest of his sustained efforts, introducing absolute temperature, obtained publicity in the Cambridge *Proceedings*, it has been judged pardonable to affix here (pp. 54–56) as a cognate *Addendum* a brief constructive essay, suitable to a more mature stage of the subject, on the Carnot-Kelvin aspect of Thermodynamics.

TRIN. COLL., *Feb.* 20, 1854.

DEAR THOMSON

Now that I have entered the unholy estate of bachelorhood I have begun to think of reading. This is very pleasant for some time among books of acknowledged merit wh one has not read but ought to. But we have a strong tendency to return to Physical Subjects and several of us here wish to attack Electricity.

Suppose a man to have a popular knowledge of electrical show experiments and a little antipathy to Murphy's Electricity, how ought he to proceed in reading & working so as to get a little insight into the subject wh may be of use in further reading?

If he wished to read Ampère Faraday &c how should they be arranged, and at what stage & in what order might he read your articles in the Cambridge Journal?

If you have in your mind any answer to the above questions, three of us here would be content to look upon an embodiment of it in writing as advice.

I have another question from myself. At Ardmillan while bathing on the rocks you mentioned that Gauss(?) had been investigating the bending of surfaces and had found in particular that the product of the principal radii of curvature at any point is unchanged by bending.

I have no means here of finding the paper from wh you quoted so that I w^{d} be obliged to you if you could give me some reference to it or even tell me whether he had considered the conditions of bending of a finite portion of a surface in general.

I have been working for some time at the more general problem & have completed the theory for surfaces of revolution and got several results in the case of other surfaces by the consideration of two systems of lines on the surface wh may be called lines of bending.

These being given the effect of bending the surface is reduced to the consideration of one indep^{t} var^{ble} only.

These lines themselves however are subject to certain conditions that the surface may be bent at all, and to additional conditions that these lines may continue "lines of bending".

When the lines of bending are the lines of principal curvature the conditions are very simple & are fulfilled for all surfaces of revol^{n}.

But some of the operations are long so I am going over them by a process quite different from the first.

By finding what Gauss' results are I may be spared much trouble in pruning my calculations.

Finally I have heard nothing of C. J. Taylor since he went to Glasgow, and w[d] gladly receive information about him, Ramsay & the professorship. Are there any *good* classical places about Glasgow or elsewhere, "where a man might enjoy a comfortable house".

This is a letter of questions so I go on in the same spirit to the end by enquiring after the prosperity of the College especially No[s] 2 & XIII i.e. commend me to the Blackburns & Mrs Thomson.

Yrs truly

J. C. MAXWELL.

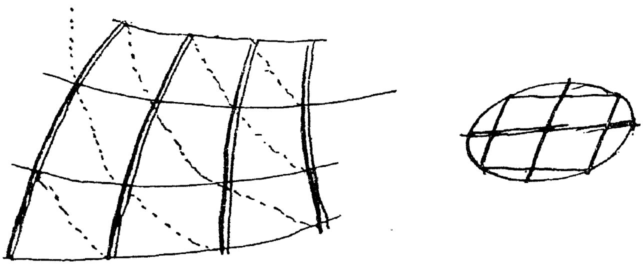
TRIN. COLL., 14 *March* 1854.

DEAR THOMSON

I have to acknowledge your letter of information, & an Exam[n] Paper. I have looked up your references, and have also got a reference to Gauss' Memoir from Stokes, who got it from Cayley. It is in Liouville's edition of Monge's Analyse Appliquée, as an addition to the theory of Surfaces. There is also an account of the researches of Bertrand, &c with further developments of Liouville's own. Both Gauss' method of referring the surface to a sphere and Bertrand's of describing a small re-entering curve on the surface had occurred to me, & I had worked them out before I saw them elsewhere. But I think the method which I had adopted at first better suited for the investigation of the particular problem of *bending*, as giving it more distinctness. You mentioned an elementary proof of your own which you thought might form a proposition in educational treatises. I sh[d] be much obliged if you could send it or mention the principal steps, if they are easily described.

Stokes asked me to fill up a blank in the Phil. Society's proceedings and so I delivered a vivâ voce exposition of bending last night, & I suppose I must proceed to put the subject into a definite form, so any assistance from you wd be acceptable.

As you approve of the reduction of problems to their elements I send you the steps of my analysis of the problem which you will easily follow as they are simple geometrical facts, all well known.



The surface is considered as the limit of the inscribed polygon and the first step is to inscribe a polygon with triangular facets.

(1) This is done by drawing on the surface 2 systems of curves forming quadrilaterals, & joining the diagonals of these by the dotted system, then, drawing plane triangles with same angular points, we have the polyhedron required.

(2) Then it is shewn that if any of these facets be produced it will cut the surface in a curve wh: is ultimately a conic section. I have called this curve the "Conic of Contact". It is a bad name, can you devise one instead?*

(3) The condition of pairs of triangles forming plane quadrilateral facets is shown to be that lines of the 1st & second system form conjugate diameters of the conic of contact at their intersection. The two systems are then said to be "conjugate to one another on the surface".

(4) Normals to four contiguous facets are drawn thro the centre of a sphere whose rad. is unity, their intersections with the sphere form the angles of a spherical quadrilateral whose area is $2\pi-(a+b+c+d)$, [thus] $\frac{a}{b}\Big|\underset{\lambda}{\underline{\;l\;}}\Big|\frac{d}{c}$, ab &c being the plane angles forming a solid angle of the polyhedron & ∴ invariable. But since a & c, b & d are ultimately equal the figure is a small parallelogram and we may find its area as such.

If l be the angle of the planes ab & λ of bc

$$\text{area} = l\lambda \sin a$$

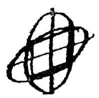
and this is easily shown to be

$$\frac{\text{area of a facet}}{\rho_1\rho_2}$$

$\rho_1\rho_2$ being principal radii of curvature. Hence when the polyhedron is bent this is invariable.

(5) When two surfaces are given one of wh: is formed from the other by bending, one & only one pair of systems of lines can be drawn which shall be conjugate to one another in both surfaces.

For let the surfaces touch in corresponding points so as to have corresponding lines in the same direction then the two conics of contact will intersect in 4 p^{ts} forming angles of a parallelogram. Lines parallel to the sides are the only ones wh: are conjugate diameters to both conics, and 2 systems of curves whose directions at every p^t correspond to these lines are the systems required. Such lines are the "*Lines of Bending*" corresponding to the given change of form of the surface. They are perfectly definite and when drawn the bending about them may be completely expressed in terms of one var^{ble}.



(6) In order that a pair of conjugate systems of lines may be lines of bending

* Cf. the cognate indicatrix of the French geometers.

at all one condition connecting their directions &c is necessary and that they may *continue* lines of bending 2 conditions are required. Hence the distinction of Instantaneous & Permanent lines of bending the Lines of Bending in 5 correspond to that axis about wh: we may suppose a rigid system turned about a point from any one pos^n into any other. Such a motion however may not be possible in every case. The true motion is about a system of Instantaneous lines wh: continually change their pos^n during the transformation.

(7) The principal lines of curvature in surfaces of revolution are shown to be permanent lines of bending and examples are given.

(8) The lines of curvature in surfaces in wh: $(\rho_1\rho_2)$ is *const* are permanent lines of bending. When $\rho_1\rho_2$ is positive $\&=a^2$ the surface may be transformed into a sphere, radius $=a$. When $\rho_1\rho_2=-a^2$ the surface may be transformed in an infinite number of ways into the surface of revolution generated by the equitangential curve

The latter prop^n is taken from Liouville.

The demonstration is original and of the same kind with the rest of the investigation.

I have also a method of defining surfaces generated by a straight line by 4 indep^t variables.

Let the eq^n to the s^t line contain one variable t.

Draw 3 consecutive lines and 2 shortest distances.

Let dt be the var^n of t

dz the shortest distance

$d\phi$ the angle between consecutive lines

ds the distance between consecutive shortest distances

$d\psi$ the angle between consec. shortest distances.

Eliminating t, we obtain ϕ, s & ψ in terms of z. Of these z ϕ & s are invariable but ψ may be altered in any arbitrary manner by bending the surface along straight lines.

So much for Mathematics.

I know one Daltonian very well but I have not seen as much as I expect to see of him yet.

The defect consists in seeing only one gradation of colour in the spectrum. Colour *as perceived by us* is a function of three independent variables at least three are I think sufficient, but time will show if I thrive.

To the Daltonian Colour is a function of two indep^{t} variables which we may call blue & yellow, so that to him colours are blue or yellow & dark or light. Red & green are between blue & yellow & may be matched in pairs.

But this is only possible under a given kind of incident light for if a red & a green are to him the same by day light, the red will be brightest by candle light. I have enabled such a man to distinguish reds & greens by showing him the opposite effects of red & green glasses upon colours similar to him. A pair of glasses, one red, one green, for the two eyes might become by practice a means of habitually distinguishing colours.

But the subject of colour as perceived by the eye is one which I intend to investigate more fully. I have satisfied myself that by using the aid of others in every experiment, and shutting out obvious sources of error, very exact results may be obtained about the equivalence of two colours as regards the eye. It is possible for people to agree on such points and this is the condition of a science of sensible colour independent of individual peculiarities. Do you know any place short of Munich where tolerable prisms could be got for chromatic purposes, just good enough to show one or two reference lines in the spectrum, and of sufficient size to transmit a pencil of $\frac{3}{4}$ inch square?

Have you seen Hopkins' pamphlet on Public Lectures for Mathematical Men entitled Remarks on the Mathematical Studies of Cambridge?

I am glad to hear (from you) that I am to be in Glasgow at Easter. I suppose when people say things they may come true. However I certainly intend to spare some time from home to visit the University of Glasgow.

Yours truly

J. Clerk Maxwell.

Trin. Coll., *Nov.* 13, 1854.

Dear Thomson

I have heard very little of you for some time except thro' Hopkins & Stokes, but I suppose you are at work in Glasgow as usual. Do you remember a long letter you wrote me about electricity, for wh: I forget if I thanked you? I soon involved myself in that subject, thinking of every branch of it simultaneously, & have been rewarded of late by finding the whole mass of confusion beginning to clear up under the influence of a few simple ideas. As I wish to study the growth of ideas as well as the calculation of forces, and as I suspect from various statements of yours that you must have acquired your views by means of certain conceptions which I have found great help, I will set down for you the confessions of an electrical freshman.

I got up the fundamental principles of electricity of tension easily enough. I was greatly aided by the analogy of the conduction of heat, wh: I believe is

your invention at least I never found it elsewhere. Then I tried to make out the theory of attractions of currents but tho' I could see how the effects could be determined I was not satisfied with the form of the theory which treats of elementary currents & their reciprocal actions, & I did not see how any general theory was to be formed from it. I read Ampère's investigations this term & greatly admired them but thought there was a kind of ostensive demonstration about them wh: must have been got up, after Ampère had convinced himself, in order to suit his views of philosophical inquiry, and as an example of what it ought to be. Yet I believe there is no doubt that Ampère discovered the laws & probably by the method wh: he has given. Now I have heard you speak of "magnetic lines of force" & Faraday seems to make great use of them, but others seem to prefer the notion of attractions of elements of currents directly. Now I thought that as every current generated magnetic lines & was acted on in a manner determined by the lines thro wh: it passed that something might be done by considering "magnetic polarization" as a property of a "magnetic field" or space and developing the geometrical ideas according to this view. I use the word "polarization" to express the fact that at a point of space the south pole of a small magnet is attracted in a certain direction with a certain force. "Polarity" is a property of magnets &c to produce pol^{n}. Then come two definitions wh: may be modified afterwards.

(1) The pol^{n} of a curve in space is the integral of the pol^{n} at any point resolved along it mult: into the element of the curve.

(2) The pol^{n} of a surface is the integral of the pol^{n} resolved in the normal into the element of surface.

The pol^{n} of a curve or surface arising from several sources is the algebraic sum of the effects taken separately.

It appears from experiment that the mag: effect of a current forming a helix of small uniform section at any point may be reduced to two forces of opposite kinds directed to the extremities and $\propto \frac{1}{d^2}$.

Whence it is easy to find the effect of a small circular current on a point at some distance from it. When forces act centrally according to the law of inverse square the pol^{n} of any closed curve is zero & that of any closed surface not passing thro the magnetic substance is zero.

Now let a number of little circuits be disposed so as to cover a given portion of a surface then the effect on any external point will be the same as that of a uniformly magnetized lamina (see your paper) and if we remove one of the circuits & draw a closed curve so as not to pass thro any one, its pol^{n} will be zero. But if we replace the circuit the pol^{n} will depend entirely on the strength of the circuit wh: thus is linked into the closed curve & will measure the intensity of the current as may be easily shown.

But in this case we may suppose that all the contiguous currents of the small circuits destroy each other, leaving the current round the boundary. Hence these theorems

(1) The pol$^{\text{n}}$ of any closed curve is measured by the sum of the intensities of all the currents which pass thro' it.

(2) The pol$^{\text{n}}$ of any surface, round the boundary of wh: a current passes, is measured by the intensity of that current.

On any of the "equipotential surfaces" (or surfaces perp to the lines of force) draw two systems of curves so as to divide the surface into small elements, the pol$^{\text{n}}$ of each of wh: is the same. Let lines of force move so as to pass thro' these curves so as to generate what I call ("surfaces of no pol$^{\text{n}}$") then these surfaces will intersect other equipotential surfaces forming elements of the same amount of pol$^{\text{n}}$ as before. Finally let a series of equipotential surfaces be drawn so that the diff. of potential is the same between each.

All space is now cut up into elements. Call the intersections of the surfaces of no pol$^{\text{n}}$ "lines of pol$^{\text{n}}$" then

(1) The pol$^{\text{n}}$ of a surface is expressed by the *number* of lines wh cut it.

(2) The pol$^{\text{n}}$ of a line is expressed by the number of equip$^{\text{l}}$ surfaces wh: it cuts.

The work done by any displacement of two circuits is measured by the increase of the number of lines of pol$^{\text{n}}$ wh pass thro both circuits.

Theory of Currents

Let u, v, w be the vel$^{\text{ies}}$ of a current in xyz that is, the quantity of electricity wh passes across unit section in unit time. $X\ Y\ Z$ electromotive forces, k coeff$^{\text{t}}$ of resistance p electric tension at any point

$$dp = (X - ku)\,dx + (Y - kv)\,dy + (Z - kw)\,dz$$

also
$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz}\,[=0].$$

From these notions we get the following which correspond to what we had in magnetism. Surfaces of equal tension cutting lines of electric motion at right angles. Electric motion along any curve made up of the sums of electric motions along its elements. Electric motion thro a surface made up of electric motion thro each element. When the surface is closed the whole electric motion is zero.

Electromotive forces

The electromotive force along any line is measured by the number of lines of pol$^{\text{n}}$ wh that line cuts in unit of time. Hence the electromotive force round a given circuit depends on the decrease of the number of lines wh: pass thro it in unit of time, that is, on the decrease of the whole pol$^{\text{n}}$ of any surface bounded by the circuit.

I have applied this to the case of a thin conducting sphere revolving about an axis placed east & west. I find that the currents are circles about an axis perp to axis of rotation & to the line of dip, and thus the vel^y varies with the distance from this axis. The magnetic effect is uniform within the sphere and the external effect may be simply expressed. I have not yet calculated the effect of the magnetism on itself when the spherical shell is thick but I see how to do it.

Induction in soft Iron &c

In these bodies a polarity is developed in each particle proportional to the pol^n at that point. The result is that the lines of force converge like the lines of heat near good conductors. The perfection of the property is the same as that of a perfect conductor of electricity, for at any point in the interior of perfectly soft iron the resultant pol^n is zero. It is easy to demonstrate the effect of iron ores for electromagnets &c in this way.

I have put the electric state of my mind before you that you may see how I am trying to make everything cohere, perhaps prematurely, & that you may know the kind of answers I want to my enquiries.

(1) Have you published any general theory founded on a theorem you gave in the *Math. Journ.* about the possibility of finding V so as to fulfill a condition given in terms of V & α

$$\frac{d}{dx}\left(\alpha^2\frac{dV}{dx}\right)+\frac{d}{dy}\left(\alpha^2\frac{dV}{dy}\right)+\frac{d}{dz}\left(\alpha^2\frac{dV}{dz}\right)=0\,?$$

(2) Is Weber's theory of the galvanic circuit to be read or can it be got?

(I am looking into Neumann in the Berlin Academy for 1847, also into Ohm's tract. I find some things by Kirchhoff on currents in solid conductors wh: are plain enough.)

(3) Can the case of ordinary electric phenomena be considered as an extreme case of conduction when the tension is enormous & the conductor excessively bad? I have not made it out yet or been able to see why there sh^d be strong attraction along the lines of almost no conduction.

(4) Can you recommend any other places to seek for further information?

This is a long screed of electricity but I find no other man to apply to on the subject so I hope you may not find it difficult to see my drift.

I have made acquaintance with Smith of Peterhouse, but only lately. Stokes has gone to London to lecture for two terms, but will return in May. I have 5 pups [=pupils] at present two of them very good, one in dynamics of particle & the other in rigid dynamics.

I am going to Cheltenham (previous to coming to Scotland) to examine the College there in Math: so I must be getting up papers and preparing for vivâ

voce, but I have not heard the subjects yet. I am glad to hear that Prof. Forbes is back to his class. I have not seen him since he has been ill. I am getting up my theory of vision of colours with the help of few Daltonians & a great many ordinary men here. Who are the Daltonians in Edinburgh that Wilson mentioned? I shd like to know a few more, so as to get greater accuracy. Remember me to No. 13 & the College generally.

Yours truly
J. C. MAXWELL

TRIN. COLL. *May* 15/55

DEAR THOMSON

Many thanks for your list of Electrical matter. I think I can get hold of all you mention. I am reading Weber's Elektrodynamische Maasbestimmungen which I have heard you speak of. I have been examining his mode of connecting electrostatics with electrodynamics, induction &c & I confess I like it not at first. He makes the attraction of two elements of electricity =

$$-\frac{ee'}{r^2}\left(1-a^2\left(\frac{dr}{dt}\right)^2+b\,\frac{d^2r}{dt^2}\right)$$

determining a & b from Ampère's laws.

But I suppose the rest of his views are founded on experiments which are trustworthy as well as elaborate.

I am trying to construct two theories, mathematically identical, in one of which the elementary conceptions shall be about fluid particles attracting at a distance while in the other nothing (mathematical) is considered but various states of polarization tension &c existing at various parts of space. The result will resemble your analogy of the steady motion of heat. Have you patented that notion with all its applications? for I intend to borrow it for a season, without mentioning anything about heat (except of course historically) but applying it in a somewhat different way to a more general case to which the laws of heat will not apply.

By the way do you profess to account for what becomes of the vis viva of heat when it passes thro' a conductor from hot to cold?* You must either modify Fourier's laws or give up your theory, at least so it seems to me.

With respect to Colours—I have been thinking of what you say about brown. I have matched ground coffee tolerably though the surface is bad—chocolate cakes & improvised various browns with black, red & a little blue & green. The only brown *paper* I have is Colcothar brown. I find

$$53\ \mathrm{Bk}+\underset{\text{Vermillion}}{28\ \mathrm{V}}+\underset{\text{Ultramarine}}{8\ \mathrm{U}}+\underset{\text{Emerald green}}{11\ \mathrm{EG}}=100\ \text{Colcothar}$$

* This belongs to the period of Thomson's perplexity about thermodynamics. See *Excursus*, pp. 54–56.

At the same time I made the following equations

$$78 \text{ Colc} + 11 \text{ U} + 11 \text{ EG} = 12 \text{ W}$$

$$36 \text{ V} + 29 \text{ U} + 35 \text{ EG} = 24 \text{ W}$$

Multiplying the first by 2 & subtracting

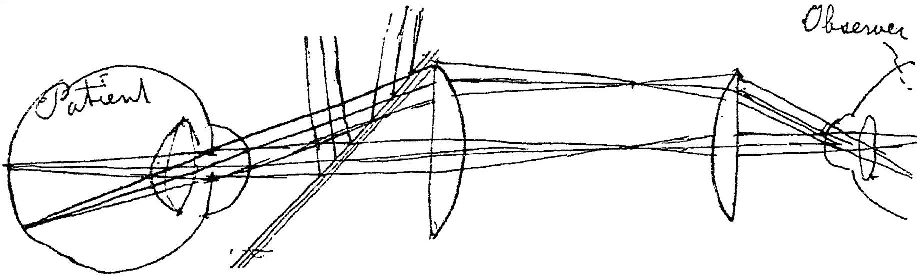
$$156 \text{ Colc} = 36 \text{ V} + 7 \text{ U} + 13 \text{ EG}$$

or

$$100 \text{ Colc} = 24 \text{ V} + 5 \text{ U} + 9 \text{ EG}$$

which is a rough approximation to the other & is good considering the light &c & the character of my helper. All my experience of coloured papers has led me to think that it only requires adjustment to make an equation with any four colours & black: but in order to preserve the identity the presence of red curtains &c should be avoided & the top viewed at a constant angle (vertically is best).

I have constructed an Eye-Speculum on Helmholtz principle but with convex glasses



The patient looks at the image of the candle as reflected by the oblique glass plates. When he sees it distinctly the rays from the image on his retina come out as they went in and passing thro the plates form an image between the lenses wh. is observed by an eye at an eyehole. The advantage of this arrangement is that the eyehole is conjugate focus to the pupil of the patient & therefore receives all the light which returns thro the pupil. In this way I have seen the image of a candle of a dark brown colour in many men's eyes, & traced some of the blood vessels. In a dog's eye I saw the brilliant colours of the tapetum with all its reticulations. This is really a beautiful object & by no means difficult to be seen. The dog does not seem to object.

I have been investigating fluid motion with reference to stability and I have got results when the motion is confined to the plane of *xy*. I do not know whether the method is new.* It only applies to an incompressible fluid moving in a plane.

* The latter part of this letter is included because it reveals Maxwell's early concern

Stability of Fluid Motion

Let the velocities parallel to x & y at the point (x, y) be

$$u = \frac{d\,(x)}{dt}, \quad v = \frac{d\,(y)}{dt} \tag{1}$$

then since the fluid is incompressible the eq^n of continuity becomes

$$\frac{du}{dx} + \frac{dv}{dy} = 0 \tag{2}$$

$\therefore$

$$v\,dx - u\,dy$$

will be a perfect diff^l of a f^n of x & y, say ψ so that

$$\left.\begin{aligned} u &= -\frac{d\psi}{dy} \\ v &= \frac{d\psi}{dx} \end{aligned}\right\} \tag{3}$$

The velocity of the fluid across the curve

$$\psi = \text{const} \tag{4}$$

is

$$u\,\frac{d\psi}{dx} + v\,\frac{d\psi}{dy}$$

which vanishes by eq^n (3) so that the curves (4) are the paths of the particles of the fluid, & may be considered as partitions in the fluid which constrain & completely determine its motion. We have also

$$\frac{dp}{dx} = \left(X - \frac{d^2(x)}{dt^2}\right), \quad \frac{dp}{dy} = \left(Y - \frac{d^2(y)}{dt^2}\right) \tag{5}$$

where X & Y arise partly from external attractions in wh.

$$X\,dx + Y\,dy + Z\,dz = d\,V$$

and partly from the reaction of the partitions.

(in 1855) with Stokes' hydrodynamic investigations of 1842–7, reprinted in *Math. and Phys. Papers*, vol. I, pp. 1–235. In this very extensive work, including his classical Report on Recent Progress in Hydrodynamics, Stokes had curiously missed, as has been remarked often, the fertile physical principle which lay exposed in the velocity potential theory of Lagrange with which he was closely concerned, namely that what he named rotational motion remained always confined to the same parts of the fluid, as is indeed stated explicitly in his Report on Hydrodynamics (1846), cf. Reprint, p. 160. That train of thought was soon originated and fully developed in the momentous classical memoir of Helmholtz on vortex motion which appeared two years later, in 1857: this memoir was translated by Tait ten years later, with experiments in air which immediately gave rise to the Kelvin outburst of generalized hydrodynamic theory, with its dynamical field of vortex atoms. Maxwell soon tried to translate these ideas into a dynamical theory of magnetism as described *infra* (cf. p. 36). The source of his knowledge at this early period was doubtless Lagrange's *Mécanique Analytique* and Stokes' early papers.

By putting
$$\frac{d^2(x)}{dt^2}=u\frac{du}{dx}+v\frac{du}{dy}$$
&
$$\frac{d^2(y)}{dt^2}=u\frac{dv}{dx}+v\frac{dv}{dy}$$
in eq^{ns} (5) and observing that
$$\frac{d}{dy}\frac{dp}{dx}=\frac{d}{dx}\frac{dp}{dy}$$
$$-\frac{dX}{dy}+\frac{dY}{dx}=-\frac{d\psi}{dy}\frac{d}{dx}\left(\frac{d^2\psi}{dx^2}+\frac{d^2\psi}{dy^2}\right)+\frac{d\psi}{dx}\frac{d}{dy}\left(\frac{d^2\psi}{dx^2}+\frac{d^2\psi}{dy^2}\right).$$
Put
$$\chi=\frac{d^2\psi}{dx^2}+\frac{d^2\psi}{dy^2}$$
then the twisting force on the element at xy
$$\frac{dY}{dx}\quad\frac{dX}{dy}=u\frac{d\chi}{dx}+v\frac{d\chi}{dy}$$
$$=\frac{d\,(\chi)}{dt}$$
where $\frac{d\,(\chi)}{dt}$ refers to the variation of χ as we pass along the path of the particle.

Since the external forces have no power of twisting this force is due entirely to the reaction of the partitions, and therefore when there are no partitions
$$\frac{d\,(\chi)}{dt}=0$$
or
$$-\frac{d\psi}{dy}\frac{d\chi}{dx}+\frac{d\psi}{dx}\frac{d\chi}{dy}=0$$
whence
$$\chi=f\,(\psi)$$
which is the condition of steady motion as is otherwise known.

If this motion be stable, then if we give the partitions a derangement the *twisting force* of the fluid on the partitions will be in the opposite direction to the twist they have received.

By making $\psi=$ f^{n} of x, y & h

we may express any change of the form of ψ by changing the value of h.

Then the twisting force of the fluid on the partitions
$$N=\frac{dX}{dy}-\frac{dY}{dx}=-\left(\frac{d\,(\chi)}{dt}+\frac{d}{dt}\left(\frac{d\chi}{dh}\right)dh\right)$$
the inclination of partition to axis of x is
$$\theta=\tan^{-}\frac{\frac{d\psi}{dx}}{\frac{d\psi}{dy}}$$

$$\therefore\ d\theta = \frac{\dfrac{d\psi}{dy}\,\dfrac{d^2\psi}{dx\,dh} - \dfrac{d\psi}{dx}\,\dfrac{d^2\psi}{dy\,dh}}{\left(\dfrac{d\psi}{dx}\right)^2 + \left(\dfrac{d\psi}{dy}\right)^2}\,dh$$

$$= -\frac{u\,\dfrac{d}{dx}\left(\dfrac{d\psi}{dh}\right) + v\,\dfrac{d}{dy}\left(\dfrac{d\psi}{dh}\right)}{u^2+v^2}\,dh$$

$$= -\frac{\dfrac{d}{dt}\left(\dfrac{d\psi}{dh}\right)}{u^2+v^2}\,dh$$

the sign of which must be the reverse of N whatever be the values of $\dfrac{d\psi}{dh}$ & of dh that the motion may be stable.

Hence (A) $$\frac{d\chi}{dt} = 0 \quad \text{or} \quad \chi = f(\psi)$$

& $\therefore$ $$\frac{d\chi}{dh} = f'(\psi)\,\frac{d\psi}{dh}.$$

So that since $\dfrac{d}{dt}\left(\dfrac{d\psi}{dh}\right)$ & $\dfrac{d}{dt}\left(\dfrac{d\chi}{dh}\right)$ must have opposite signs and since we suppose the displacement of the partitions to begin after a certain time.

(B) $f'(\psi)$ or $\dfrac{d\chi}{d\psi}$ must be negative for stability

When $f'(\psi)$ is positive the motion is unstable.

When $f'(\psi) = 0$, χ is constant or 0.

When χ is constant I think eq^n is neutral.

When $\chi = 0$ the whole motion is determined by the motion at a limiting curve so that there can be no finite displacement.

Example

In a vortex in which the velocity is as the nth power of the distance from the centre the value of n must be between $+1$ & -1 for stability.

In 3 dimensions this method is complicated, we have

$$u = \frac{d\phi}{dy}\,\frac{d\phi'}{dr} - \frac{d\phi}{dr}\,\frac{d\phi'}{dy}$$

$$v = \frac{d\phi}{dr}\,\frac{d\phi'}{dx} - \frac{d\phi}{dx}\,\frac{d\phi'}{dr}$$

$$w = \frac{d\phi}{dx}\,\frac{d\phi'}{dy} - \frac{d\phi}{dy}\,\frac{d\phi'}{dx}, \ \&c$$

Another Method

Consider the fluid within the curve

$$u=0.$$

Its vis viva $=\Sigma\,(v^2)$ $$\iint dx\,dy\left\{\left(\frac{d\psi}{dx}\right)^2+\left(\frac{d\psi}{dy}\right)^2\right\}$$

between proper limits. Integrating by parts

$$\Sigma\,(v^2)=\int dx\,\frac{d\psi}{dx}\,\psi_{y_2}-\int dx\,\frac{d\psi}{dx}\,\psi_{y_1}$$
$$+\int dy\,\frac{d\psi}{dy}\,\psi_{x_2}-\int dx\,\frac{d\psi}{dy}\,\psi_{x_1}-\iint dx\,dy\,\psi\left(\frac{d^2\psi}{dx^2}+\frac{d^2\psi}{dy^2}\right)$$
$$=\int ds\,\psi\,\frac{d\psi}{du}-\iint dx\,dy\,\psi\left(\frac{d^2\psi}{dx^2}+\frac{d^2\psi}{dy^2}\right)$$

where ds is an element of the curve & u is measured in direction of normal.

The first term here refers to the limits & is not altered by any derangement within the limiting curve. The second term is of the familiar form

$$\iint dx\,dy\,V\left(\frac{d^2V}{dx^2}+\frac{d^2V}{dy^2}\right)$$

in which we regard V as the potential &

$$\frac{d^2V}{dx^2}+\frac{d^2V}{dy^2}=4\pi\rho.$$

Now we know that $$4\pi\iint dx\,dy\,V\rho$$

is a maximum for stable & a minimum for unstable eq^{ns}. In both cases

$$\rho=fV$$

and in the former $f'(V)$ is positive.

Hence $$\Sigma\,(v^2)=\iint dx\,dy\left\{\left(\frac{d\psi}{dx}\right)^2+\left(\frac{d\psi}{dy}\right)^2\right\}$$

is a maximum or a minimum when

$$\chi=\frac{d^2\psi}{dx^2}+\frac{d^2\psi}{dy^2}=f\,(\psi)$$

maximum when $f'(\psi)$ is negative, minimum when $f'(\psi)$ is positive.

Now if the vis viva be a maximum any derangement will diminish it & the motion will tend to revive again and vice versa.

I hope to hear again of M^{rs} Thomson's progress. I have good accounts of my father who seems quite recovered.

Yours truly

J. C. MAXWELL.

GLENLAIR, *Sept.* 13, 1855.

DEAR THOMSON

I hoped to have been in Glasgow by this time, but my departure was postponed, owing to the state of my father's health. I wrote to M^rs^ Blackburn to tell what had happened & how things were going on, so I need not repeat particulars. He is now downstairs, and apparently as well as ever, but I think I had better stay here till we see his health reestablished.

If I had seen you in Glasgow, I should have asked you some questions which have been some time in store. Perhaps it is better as it is, not to bother you with "vivâ voce" while you are busy with the wise men, but to write them down for you to answer by post during the lull between the extraordinary & the regular occupation of the College.

I have got a good deal out of you on electrical subjects, both directly & through the printer & publisher & I have also used other helps, and read Faraday's three volumes of researches. My object in doing so was of course to learn what had been done in electrical science, mathematical & experimental, and to try to comprehend the same in a rational manner by the aid of any notions I could screw into my head. In searching for these notions I have come upon some ready made, which I have appropriated. Of these are Faraday's theory of polarity which ascribes that property to every portion of the whole sphere of action of the magnetic or electric bodies, also his general notions about "lines of force" with the "conducting power" of different media for them.

Then comes your allegorical representation of the case of electrified bodies by means of conductors of heat, and your theorem on the eq^{n}

$$\frac{d}{dx}\left(\alpha^2\frac{dV}{dx}\right)+\frac{d}{dy}\left(\alpha^2\frac{dV}{dy}\right)+\frac{d}{dz}\left(\alpha^2\frac{dV}{dz}\right)=-4\pi\rho.$$

Then Ampère's theory of *closed* galvanic circuits, then part of your allegory about incompressible elastic solids & lastly the *method* of the last demonstration in your R.S. paper on Magnetism. I have also been working at Weber's theory of Electro Magnetism as a mathematical speculation which I do not believe but which ought to be compared with others and certainly gives many true results at the expense of several startling assumptions.

Now I have been planning and partly executing a system of propositions about lines of force &c which may be *afterwards* applied to Electricity, Heat or Magnetism or Galvanism, but which is in itself a collection of purely geometrical truths embodied in geometrical conceptions of lines, surfaces &c.

The first part of my design is to prove by popular, that is not professedly symbolic, reasoning, the most important propositions about V and about the solution of the equation in the last page $\left(\frac{d}{dx}\alpha^2\frac{dV}{dx}+\&c\right)$ and to trace the lines of force and surfaces of equal V.

The second part would be nothing else than a collection of examples of your "Electrical Images" worked out by the method of lines of force & equipotential surfaces, for cases of spheres of various conducting powers and for the case of a sphere of a "magnecrystallic" substance in a uniform field.

The third part would be the theory of the connections of the three divisions of the subject, the passage of statical into current electricity and the magnetic properties of closed currents with the laws of induced current.

I intend next to apply to these facts Faraday's notion of an *electronic* state. I have worked a good deal of mathematical material out of this vein and I believe I have got hold of several truths which will find a mathematical expression in the electrotonic state.

One thing at least it succeeds in, it reduces to one principle not only the attraction of currents & the induction of currents but also the attraction of electrified bodies without any new assumption.

Now what I have planned out in this way I intend to set about for the sake of acquiring knowledge sufficient to guide me in devising experiments; but supposing that at any time I should be satisfied that the theory would stick together all through, and that I could put it into a form not too abstrusely mathematical, but still exact and leading to numerical results then I would be much assisted by your telling me whether you have not the whole draught of the thing lying in loose papers and neglected only till you have worked out Heat* or got a little spare time.

The reasons I have for thinking so are—That you are acquainted with Faraday's theory of lines of force & with Ampère's laws of currents and of course you must have wished at least to understand Ampère in Faraday's sense. You had the advantage of being well acquainted with V and with Green's essay, and you published a fragment of your speculations in the form of an allegory about incompressible elastic solids. In your paper on Electrolysis you state several of the laws of induction of currents which are right as far as they are stated and at the end of your paper of magnetism you have not only stated and applied Ampère's properties of currents but used a method in your demonstration about the superficial tangential distribution of magnetism in a solenoidal magnet, which seems to me to be part of my results applied to magnetism without acknowledging that you had taken it from a more general theory.

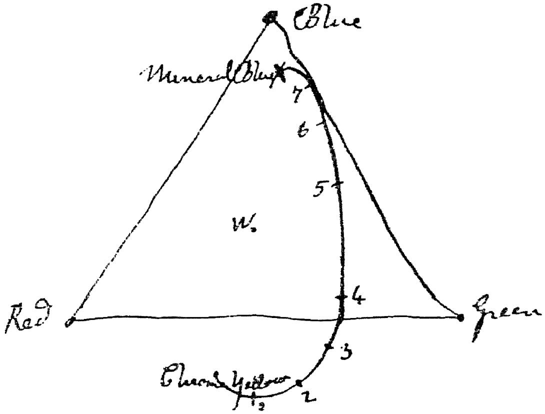
As there can be no doubt that you have the mathematical part of the theory in your desk all that you have to do is to explain your results with reference to electricity. I think that if you were to do so publicly it would introduce a new set of electrical notions into circulation & save much useless speculation.

I do not know the Game-laws & Patent-laws of science. Perhaps the Association may do something to fix them but I certainly intend to poach among

* Including his perplexities about Thermodynamics? Cf. *Excursus*, pp. 54–56.

your electrical images, and as for the hints you have dropped about the "higher" electricity, I intend to take them. At the same time if you happen to know where anything on this part of the subject is to be found it would be of great use to me.*

Since I last wrote to you I have been making mixtures of colours by weight and finding the resulting colour by means of the colour-top. I find that very few mixtures lie in the line joining their components, but that they form a curve generally pretty regular from the one to the other. In the case of Chrome Yellow & Mineral Blue (copper I believe) the curve goes away among the greens.



I have a good many other pairs of colours, but this is the most remarkable.

I am beginning to get results on the colours of the spectrum, but I have great difficulties in getting my apparatus sufficiently steady & accurate & in measuring the diameters of slits.†

My father was up to breakfast today but we must take care that he does not do too much. How is Mrs Thomson? I have heard nothing of either of you since Malvern. Dr G. Wilson is to be at Glasgow. I have had several letters from him on Colour blindness.

I shall be here till the 24th Sept. after which address Trin. Coll. If I do not hear from you soon I hope to hear later.

Yours truly

J. C. MAXWELL.

* When Faraday re-discovered a specific static induction across dielectrics, to the detriment of current ideas regarding action across a distance, he was delighted to find how promptly W. Thomson subsumed it under the general theory of polarized media, as later systematized in his development of the Poisson theory of magnetization; this initiative became refined and expanded in time into Clerk Maxwell's scheme of gradual electric transmission (pp. 35–39 *infra*). Cf. Thomson's earlier papers, now strangely neglected, in the reprint of *Papers on Electrostatics and Magnetism* (1872): in its preparation the author recorded assistance from Clerk Maxwell and P. G. Tait.

† Maxwell does not here refer to the Young-Helmholtz trichromatic theory of vision. But according to Helmholtz (1868) the exact numerical laws of composition, on which it and all other theories must repose, were the discovery of Maxwell Cf. an illuminating account in his *Popular Scientific Lectures*, vol. i, pp. 214, 219.

TRIN. COLL., 14 *Feb.* 1856.

DEAR THOMSON

I left a paper with you on "Faraday's lines of Force". If you have done with it & can lay hands on it, I would like to have it, because I have to write up the second part—"On Faraday's Electrotonic State", and there will be a necessity for reference. I think I left an abstract too. Tell me if you have it, but do not seek for it if you do not know where it is. I was working at the following problems in the vacation.

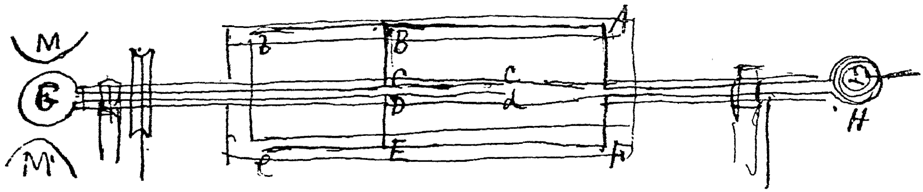
I. A thin hollow shell of conducting matter whose resistance and magnetic coeff^{t} are given revolves about any axis in a uniform field of magnetic force. To find the electric currents and the magnetic effects (the magnetism due to the electric currents changes the magnetic field so that the resultant magnetism depends in direction as well as quantity on the velocity of rotation).

II. A closed wire of circular section is placed in the neighbourhood of another circuit through which a current whose intensity at any time is known is sent. To find the current at any time in the closed wire, regard being had *to the induction of the wire on itself.*

I find that if we know by experiment the potential of the one circuit on the other, supposing each to be traversed by a unit current, and also the potential of each on itself, then we get rid of all the integrations and we have only to consider the peculiar effects due to the difference of the electromotive forces at different parts of the section of the wire. These are found by a very rapid approximation.

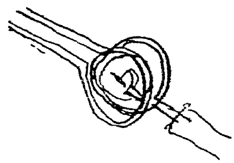
III. A closed wire is made to rotate about an axis so rapidly that it produces a constant deflection of a needle in its neighbourhood. To find the relation between the velocity &c and the deflection of the needle.

I think something might be made of the following plan of determining the amount of magnetic induction passing through a given closed curve.



MM' are magnetic poles forming a field at *G*, *cCGDd* is a wire attached to a horizontal axis and revolving with it. *eEFHABb* is another wire attached also to the same axis. *BC*, *DE* are moveable links connecting one of these wires with the other and so completing the circuit. *H* is a coil consisting of a small

number of turns of thick wire and allowing a small magnetic needle to be introduced suspended by a hook from a fixed rod in the axis produced and protected from wind by a glass globe. Let the horizontal axis be north & south, then the effect of turning the apparatus will be to produce a current alternating in direction as respects the wire but always in the same general direction in space. If the rotation be rapid the deflection of the needle at H will be due to the difference of the inductive magnetic action of the magnets through G and that of the earth through $ABEF$. Now by moving BC, DE we can vary the area $ABEF$ so as to make the whole effect 0. Then the number of the magnet's lines through G will be equal to the number of earth's lines through $ABCDEF$.



I have been working at the theory of Optical Instruments and I have reduced the whole theory of Geometrical Foci of compound instruments to a few propositions founded on the following axioms.

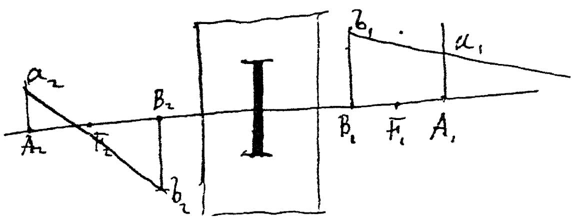
I. If a pencil of light falling on an instrument be small, nearly centrical and nearly direct, then if the incident light have a focus the emergent light will have a determinate focus.

II. The distances of any two given rays of a pencil from its axis at any section before incidence are proportional to their distances in any other section after emergence.

Definition

If a small cylindrical pencil of light fall directly on an instrument, its form after emergence will be a double cone. The vertex of this cone is the *principal focus* and the true sections which are equal to that of the cylinder give the positions of two *focal centres*. That in which the rays are on the same side of the axis as in the cylinder is called the *positive focal centre*, and the other the *negative focal centre*. Turning the instrument the other way we get another principal focus and two focal centres.

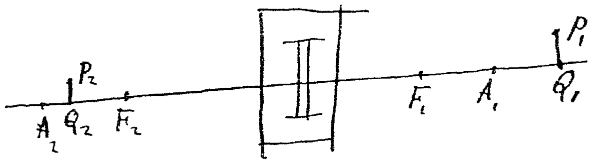
From these axioms, without any assumption of law of refraction, centres of lenses &c we get this construction for any ray.



Let A_1 A_2 be the positive & B_1 B_2 the negative focal centres of the instrument I. Then if $a_1 b_1$ be any ray whatever cutting the planes of the focal centres in $a_1 b_1$. Make $A_2 a_2 = A_1 a_1$ on the same side of the axis and $B_2 b_2 = -B_1 b_1$ on the opposite side of the axis. Join $b_2 a_2$ for the direction [path] of the emergent ray.

We have also the following rule for conjugate foci.

Let A_1 A_2 be positive focal centres, F_1 F_2 principal foci, P_1 P_2 conjugate foci for incident and emergent rays



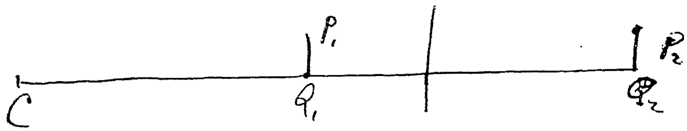
$$\frac{F_1Q_1}{F_1A_1}=\frac{F_2A_2}{F_2Q_2}=\frac{P_1Q_1}{P_2Q_2}$$

or
$$F_2Q_2=\frac{(F_1A_1)\,(F_2A_2)}{F_1Q_1},\quad P_2Q_2=\frac{(F_1A_1)\,(P_1Q_1)}{F_1Q_1}$$

so that the position of P_2 may be found from that of P_1 by multiplication & division only.

I have also considered the laws of combination of two instruments and the case in which they form a "telescope", in which the focal centres &c go off to infinity and instead we have a "centre" C a "modulus" n and a "linear magnifying power" m.

We have then

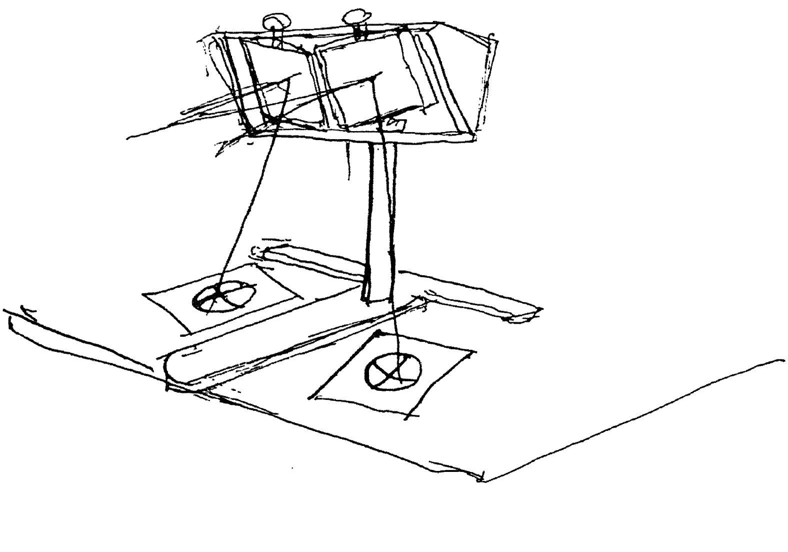


$$CQ_2=nCQ_1$$
$$Q_2P_2=mQ_1P_1$$

for the law of conjugate foci for a "telescope".

I am beginning to apply this method to the theory of primary & secondary foci of compound instruments. I find a great many good things in Smith's "Opticks". Do you know where Möbius has put his optical theorems?

I am working at another optical question—the place where a screen must be



held so as to give the most distinct image of the edge of anything, the instrument being afflicted with aberration. It is not the "least circle of aberration": that is a very useless thing, as I find in practice.

Smith (page 86 of Vol 2, art. 526 to 530) gives a good set of illustrations of the principles of the stereoscope.

I have set up a reflecting stereoscope for Solid Geometry.

Two mirrors turning on axes inclined 45° to vertical. Pictures horizontal on table. Spectator looks horizontally.

I hope to hear a good report of the College & all its inmates. Remember me to Rankine.

Yours truly

JAMES CLERK MAXWELL

TRIN. COLL., 19 *Feb.* 1856.

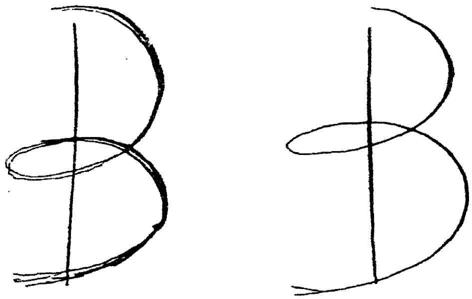
DEAR THOMSON

Thanks for your letter, written under stress of time, and for the papers on which has come no hurt.

I am glad we are to have some tangible memorial of your experiments for it appeared to me that science would suffer for want of a reporter like the worthies who lived before Agamemnon.

My present business is to trouble you again. I wish you to write out a description of me and sign it that I may send it to the representatives of the Crown and that they may look favourably on my scheme of setting up as professor of nat. phil. at Marischal College Aberdeen. I have written to the Home Sec[y] & the Lord Advocate to offer myself and I have said that I would send in Testimonials as soon as my friends could write them out. I had got promise of several but I reckoned on yours too. I trust I was right both in the whole affair and in this particular supposition. I know nothing about the time of election or the kind of candidates but time is always valuable, so the sooner you write the better.

I did not know M[r] Gray. Prof. Forbes writes to me that he was a young & healthy man and known to him.



If M[r] Joule can see stereoscopic pictures, putting a division from his nose to between the pictures, he can do more than I can. If he squints with the right eye at the left picture et v.v. he sees everything inside out. Which?

These are two precisely similar curves projections of the helix. If you squint at them straight they will appear plane curves. If you raise the left side of the paper you will see a right handed helix start into solidity and if you raise the right the helix will be left handed (squinting across).

With well drawn curves the effect is wonderful.

I have drawn stereoscopic pictures of the spherical ellipse in all positions of a sphere with two cylinders crossing it (the "Florentine Enigma") of a knot on a thread and a collection of others all large for the reflecting Stereoscope.

Yours
J. C. MAXWELL

TRIN. COLL., *Feb.* 22, 1856.

DEAR THOMSON

I return you Fuller's letter; I had written to him myself before I got it, to ask for information, particularly the time of election. He says that Tait is in. He is doing well at Belfast & is resolved to do something in physics. I should not be sorry to see him get some encouragement. I do not think his own college appreciates his mathematical powers. There are two more men I know Swan, a teacher in Edin^{r} who has done several ingenious things in various subjects, and MacLennan a Trinity man, now in Edin^{r} who would get on as a lawyer better than as a professor.

Both have asked me for my good will which they have as I do not think either of them men to be avoided and both are very ingenious & MacLennan, though very loose in some parts of science has a great power of expression.

As for what Fuller says about the press I think that if there is a good virulent newspaper editor quarreling with the whole college, the professors may agree to quarrel with him instead of one another.

Now a chronic slanging mania is the normal state of the editor but the utter spoiling of a college. I will therefore give my vote that Carthage should be spared.

Some of my friends are going to act on Lords Ardmillan & Cowan and I hope to organise another line of attack through Sir J. Herschel & Lord Aberdeen.

Swan has sent me his testimonials neatly printed so that I shall be able to give the printer a specimen of how he ought to truss these things up. How came Cayley to be wanting a professorship.* I thought he was a great lawyer. One might suppose Adams to be wanting it much more.

I have found a curve in space that has its centre of plane curvature & centre of spherical curvature easily determined. I have made stereoscopic pictures of it and have been able to explain to the 3rd year men the whole theory of normal

* The Sadleirian Professorship at Cambridge was created for Cayley in 1863.

& osculating planes principal normal, axis of curvature involute in space & so on. The eq$^{\text{ns}}$ to the curve are

$$x = 4a\cos^3\theta, \quad y = 4a\sin^3\theta, \quad z = 3c\cos 2\theta.$$

Those of the centre of sphere of curvature

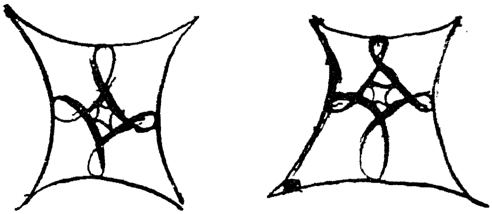
$$x = 4\,\frac{4a^2+3c^2}{a}\cos^3\theta, \quad y = 4\,\frac{4a^2+3c^2}{a}\sin^3\theta, \quad z = 3\,\frac{4a^2+3c^2}{c}\cos 2\theta$$

and those of the centre of circle of curvature are

$$x = 4a\cos^3\theta + 12\,\frac{a^2+c^2}{a}\cos\theta\sin^2\theta, \quad y = 4a\sin^3\theta + 12\,\frac{a^2+c^2}{a}\cos^2\theta\sin^2\theta$$

$$z = 3c\cos 2\theta.$$

I have learnt a great deal about curves by drawing this one.



Here is a stereoscopic view drawn by hand which will show you that in the large curve  which is the locus of centres of spheres of contact the upper left & lower right corners are uppermost while the opposite is the case with the original curve which is small . The locus of centre of $\odot$ of curvature  has the upper right & lower left sweeps uppermost. It is an involute of the large curve. I hope to hear soon of the Bakerian Lecture. Roscoe told me about it. Do you think my paper on Faraday's lines too long for the *Phil. Mag.* I would like to put it in because Faraday reads it and so does Tyndall.

Yours truly

J. C. MAXWELL.

TRINITY COLLEGE, 25 *April* 1856
[with mourning border]

DEAR THOMSON

I enclose a note to your brother, for though you told me his address I have forgotten it. Blackburn would tell you of my private affairs since we left Glasgow. My father expected his death continually and the fear of it never alarmed him. It came at last with less outward signs than had been seen on several occasions in which he recovered. He was happy to have got home, and to have accomplished several things he had on hand, and though he might have had comfort in his troubles here, he was content to go, though he did not know the time.

I returned here on the 19th as they had got no one to take my lectures. I am getting up the analytical part of my electromagnetic functions.

Have you any propositions on maxima or rather minima of quantities like

$$\iiint (a\alpha + b\beta + c\gamma)\, dx\, dy\, dz$$

through infinity where abc are measures of quantity, magnetic or galvanic, and $\alpha\beta\gamma$ of intensity and

$$a = A_1\alpha + B_1\beta + C_1\gamma$$
$$b = A_2\alpha + B_2\beta + C_2\gamma$$
$$c = A_3\alpha + B_3\beta + C_3\gamma$$

If $$\alpha = \frac{dp}{dx},\ \beta = \frac{dp}{dy},\ \gamma = \frac{dp}{dz}$$

and if $$\frac{da}{dx} + \frac{db}{dy} + \frac{dc}{dz} = -4\pi\rho$$

where ρ is a given f^n of xyz

$$\frac{d}{dx}(A_1\alpha + A_2\beta + A_3\gamma) + \frac{d}{dy}(B_1\alpha + B_2\beta + B_3\gamma) + \frac{d}{dz}(C_1\alpha + C_2\beta + C_3\gamma) = 4\pi\rho$$

is the condition of minimum.

If you have any three functions of xyz such as abc and if you make

$$\frac{da}{dx} + \frac{db}{dy} + \frac{dc}{dz} = -4\pi\rho$$

and if you find the value of V

$$V = \iiint \frac{\rho\, dx\, dy\, dz}{\sqrt{(x-x')^2 + (y-y')^2 + (z-z')^2}}$$

and make $$a - \frac{dV}{dx} = a', \quad b - \frac{dV}{dy} = b', \quad c - \frac{dV}{dz} = c'$$

then
$$\frac{da'}{dx}+\frac{db'}{dy}+\frac{dc'}{dz}=0$$
and therefore by your prop. in theory of magnetism if $a'b'c'$ be given as continuous functions of xyz and if $\int a'dz$ mean the result of integration with respect to z as the quantity stands then if

$$\alpha_0=\int c\,dy-\int b\,dz$$
$$\beta_0=\int a\,dz-\int c\,dx$$
$$\gamma_0=\int b\,dx-\int a\,dy$$

then we shall have

$$a=\frac{dV}{dx}+\frac{d\beta_0}{dz}-\frac{d\gamma_0}{dy}$$
$$b=\frac{dV}{dy}+\frac{d\gamma_0}{dx}-\frac{d\alpha_0}{dz}$$
$$c=\frac{dV}{dz}+\frac{d\alpha_0}{dy}-\frac{d\beta_0}{dx}.$$

Now if we wish to transform

$$\iiint(a\alpha_1+b\beta_1+c\gamma_1)\,dx\,dy\,dz$$

where the integration is through infinity and all the quantities vanish there and if abc are expressed as above then

$$\iiint\left(\frac{dV}{dx}\alpha_1+\frac{dV}{dy}\beta_1+\frac{dV}{dz}\gamma_1\right)+\left(\frac{d\beta_0}{dz}-\frac{d\gamma_0}{dy}\right)\alpha_1+\left(\frac{d\gamma_0}{dx}-\frac{d\alpha_0}{dz}\right)\beta_1+\left(\frac{d\alpha_0}{dy}-\frac{d\beta_0}{dx}\right)\gamma_1$$
$$=-\iiint V\left(\frac{d\alpha_1}{dx}+\frac{d\beta_1}{dy}+\frac{d\gamma_1}{dz}\right)dx\,dy\,dz$$
$$-\iiint\alpha_0\left(\frac{d\beta_1}{dz}-\frac{d\gamma_1}{dy}\right)+\beta_0\left(\frac{d\gamma_1}{dx}-\frac{d\alpha_1}{dz}\right)+\gamma_0\left(\frac{d\alpha_1}{dy}-\frac{d\beta_1}{dx}\right).$$

Now if we put
$$\iiint\frac{\left(\frac{d\alpha_1}{dx}+\frac{d\beta_1}{dy}+\frac{d\gamma_1}{dz}\right)dx\,dy\,dz}{\sqrt{(x-x')^2+(y-y')^2}+(z-z')^2}$$
then the first term becomes
$$\Sigma V\left(\frac{d^2p}{dx^2}+\frac{d^2p}{dy^2}+\frac{d^2p}{dz^2}\right)$$
which we know $=\Sigma p\left(\frac{d^2V}{dx^2}+\frac{d^2V}{dy^2}+\frac{d^2V}{dz^2}\right)$ or $=4\pi\Sigma p\rho$

and if we put $\frac{d\beta_1}{dz} - \frac{d\gamma_1}{dy} = a_2$ &c the second term is

$$\iiint \alpha_0 a_2 + \beta_0 b_2 + \gamma_0 c_2.$$

Now if $a_1 b_1 c_1$ be measures of quantity of magnetization ρ is the real magnetic density, and $\alpha_0 \beta_0 \gamma_0$ are the electrotonic functions, and $a_2 b_2 c_2$ the electric currents if $\alpha_1 \beta_1 \gamma_1$ be the intensities of magnetization so that

$$\Sigma a_1 \alpha_1 + b_1 \beta_1 + c_1 \gamma_1 = -\Sigma 4\pi p\rho - \Sigma\ (a_0 \alpha_2 + b_0 \beta_2 + c_0 \gamma_2).$$

Yours truly

J. C. MAXWELL

I seem to have got the professorship.

129 UNION STREET, ABERDEEN.
Dec. 17, 1856

DEAR THOMSON

I do not know what special subject you are busy with now. My special study is Elementary Mechanics and just at present parabolic motion. I am glad to find that the students are better pleased with Dynamics than with Statics. I was afraid that the new ideas would be more difficult than those of Couples or Friction. However they did a paper on definitions and rectilinear motion far better than they did on friction, and I had some very sensible difficulties sent up to-day about the measure of force by momentum produced, and about the dimensions of the quantities in $S = \frac{1}{2}\frac{F}{M}t^2$.

I want to hear from you about your method of making one man look over another's exercise. It would do them good but I do not yet see how to get my men into the way of it. I would like to see what sort of subjects you give & to know how you arrange the critics.

At present the hour from 9 to 10 is supposed to be oral exam^{n} and 11 to 12 lecture but I find it best to do both at both hours and examine without warning, for pure examination is tiresome for those who are not examined and pure lecturing encourages passivity in passive men, not to say talking and note-writing among the oblique minded. On Tuesdays I dictate 10 questions (with short answers) to be answered in writing on the spot. On Wednesday I explain them and hang up a list of the numbers each man has answered. They do not admit of half answers.

I also give out exercises to be done at home, of a more difficult kind, but these are voluntary, and confer no distinction: only I correct them and explain them in the class, and to such men as wish explanations.

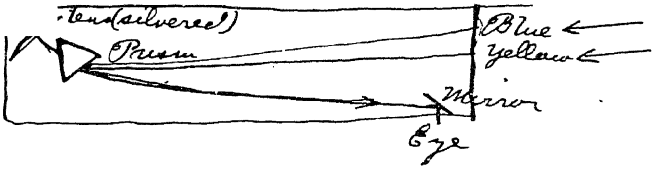
Volunteers of the fourth year come two days a week. We do Newton I II III. They have diff. Calc. afterwards from D^{r} Cruickshank.

Most of them had Whewell's Mechanics, so I have continued it this year; but couples & friction are wanting, and the proof of composition of forces is not well done even admitting all the axioms. It is better done in Poinsot's *Statique* from the lever. But men *must* learn the "transmission of force" proof. I have set it up in a concrete form and I think it is understood now. But next year I shall take Phear's book. Do you think it good? I would rather have every man his own mechanician but that is not easy. I dont intend to have a text book on hydrostatics. Optics I must consider.

I have arranged to have Geometrical Conics taught in time for Projectiles, but I must inculcate limits and increments myself. I have not used the words yet, but some of us understand the things and I shall venture on the words before long.

I should much like to hear from you something about your class and methods.

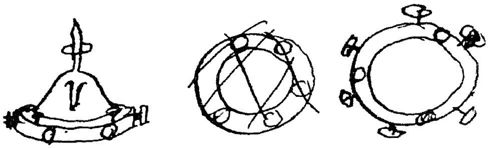
With respect to other things. I constructed in July my *short* instrument* for exhibiting compound colours with a prism.



The rays go twice through the prism and so what would need a box with two chambers four feet long at an awkward angle is done in a rectangular box 2 feet × 7 inch × 4 inch. Also the entrance of light is close to eye hole and the slits can be arranged by the observer. Bryson had it in his shop for a week and got it up as I think pretty well by heart; but I dont suppose he quite knows the adjustments. The focussing is very simple = turning the prism on its axis.

Smith & Ramage here are constructing an improved edition of my dynamical top. See report of British Association '56.

I have a better notion of the necessary dimensions and proportions and of the most convenient arrangement of the screws.



There are six horizontal & 3 vertical screws on the ring, which give the 9 adjustments 3 for centre of gravity 6 for magnitude & pos^{n} of principal axes.

* Identical in principle with the modern short astronomical spectroscope.

But for large changes I have two more adjustments—the axis screws up and down, and a bob screws on the axis. I have found a blacksmith who made a capital balance for showing stability &c and false arms, adjustible, but very strong, and fit for the class to handle. He has also made me a pendulum on gymbals which goes well and long, and can be used as a Kater's pendulum.

I have constructed a catenary 3 feet long of bits $\frac{3}{4}$ inch diam^r which will stand as an arch of any form, flat or lofty. I am also getting a machine for throwing bullets by means of a weight which I hope will be a trustworthy engine.

So much for the concrete. Here is a piece of geometry. Do you know if it is old.

Def. If from a fixed point in a plane we draw lines to every point in a plane curve and cut off from each radius a part inversely proportional to that radius, the new points form a curve and the transformation is called "Inversion". See Mulcahy, Salmon &c.

Prop^n. If the operation of inversion be performed any number of times on a plane curve, using different fixed points each time, one inversion can always be determined which shall be equivalent to the result of the whole.

Proof. The operation of inversion if performed on a stereographic projection of the globe is equivalent to changing the pole of projection to the latitude and longitude of the fixed point as given by the map. Therefore after any number of inversions the pole of projections will have a certain latitude & longitude and the scale of the map will be determined by the moduli.

But we can by a proper fixed point and modulus produce a map with given pole and scale by a single inversion. Q.E.D.

Another in particle Dynamics.

A particle is projected along the internal surface of a smooth sphere and attracted towards a fixed plane outside the sphere with a force varying as inverse cube of dist: The velocity of proj^n is that due to ∞. Show that the path of the point is a circle whose plane passes through the *pole* of the given plane.

My paper on Faraday's Lines has stuck somewhere & I am trying to find out where the latter part of the MS. is. I have proofs of all the first part. Stokes had looked over it and had pointed out some awkward blunders, a page of "divide" instead of "multiply". Did Blackburn tell you about the 9 coeff^ts of magnetic conductivity allowing perpetual motion?

Tyndall's paper on Diamagnetism is satisfactory and Faraday's 30^th Series confirms my results about magnecrystallic action in different surrounding media. Has your lecture been printed? I have been looking at Rankine's Thermo-dynamic diagrams. It is a great relief after the Umbral notation. I expect a few papers on the Oscan or Ligurian notation soon.

I am glad to hear better accounts of M^rs Thomson. I owe M^rs Wedderburn a letter, I hope young Hugh is better. I hear from W. Cay, at Belfast, who seems taking root there. I made a splendid vortex lately, quite smooth and steady and 7 inches deep in the middle.

W. A. Porter writes from Lincoln's Inn that he wonders he was ever anything else than a young lawyer. Such is life out of Peterhouse.

Yours truly
J. C. MAXWELL.

129 UNION STREET [ABERDEEN], 18 *Dec.* 1856

DEAR THOMSON

Your letter & mine crossed. With respect to the history of the optical effects of compression my belief is, that Seebeck & Brewster discovered independently about 1814 the effects of heat in "developing the depolarizing structure in glass".

Brewster about 1815 discovered the effects of compression, dilatation & induration on glass gum jelly wax—and resin &c. I do not know whether Seebeck had found the effects of a permanent kind in unannealed glass.

Fresnel made the first experiment to demonstrate the separation of the pencils by compressed glass.

If, in 1815, Brewster considered depolarizing structure equivalent to double refraction then he is the discoverer of both. Fresnel certainly is the first to prove to the eye that there is double refraction and I do not think Brewster in 1815 considered the two things identical.

Therefore as Brewster discovered the phenomenon and Fresnel developed it you had better say:

Thus Sir D. Brewster discovered that mechanical stress induces temporarily in transparent solids directional properties with respect to polarized light, and Fresnel has identified these properties with the double refraction of crystals.

With respect to the effects of heat Brewster seems to consider them due to caloric and Herschel seems to be the first to reduce them to cases of mechanical stress. See Encyc. Met., "Light".

Herschel's explanations are *very good*. He however connects the optical properties with pressure or force rather than with compression or disfigurement and therefore holds that in all cases, including jelly gutta percha wax & resin &c there must be actual pressure.

In my paper on elasticity I produced instances (not new phenomena) to show that in these cases the pressure could not exist as the effects were in the same direction over the whole body & therefore could not be balanced. At the same time I stated that gutta percha when heated to a certain point goes back to its original form.

Wertheim, *Ann. de Chimie* Jan 1854? has the best experiments I have seen

on compressed glass. He maintains & I think with truth that the effects are due to the "*strains*" not to the "stresses". As far as I know glass is not capable of maintaining strain without stress at least I cannot cut a piece out of an unannealed plate which has the strain all in one direction. When it is cut out the strain disappears for want of stress.

If you examine any plane section of a piece of unannealed glass you will find that $\Sigma dSp = 0$ where p is the stress perp. to the element dS. That is, the whole pressure = whole tension in the section.

You should not make me a partaker in Sir D. B's experiments. The *phenomena* are all due to him (except gutta percha of which I do not know the optical history) and they date from 1815 & so on.

The reduction to double refraction is Fresnel's.

That of Heat to mechanical action—Herschel.

Proof that this action is not stress—Maxwell.

Proof that it is strain with numerical data for various kinds of glass—Wertheim.

In connection with this see your brother's papers on strained wires after the stress has been removed.

Here is my present notion about plasticity of homogeneous amorphous solids.

Let $\alpha\beta\gamma$ be the 3 principal strains at any point PQR the principal stresses connected with $\alpha\beta\gamma$ by symmetrical linear equations the same for all axes. Then the whole work done by PQR in developing $\alpha\beta\gamma$ may be written

$$U = A\ (\alpha^2+\beta^2+\gamma^2) + B\ (\beta\gamma+\gamma\alpha+\alpha\beta)$$

where A & B are coeffts, the nature of which is foreign to our inquiry. Now we may put

$$U = U_1 + U_2$$

where U_1 is due to a symmetrical compression ($\alpha_1 = \beta_1 = \gamma_1$) and U_2 to distortion without compression ($\alpha_2+\beta_2+\gamma_2 = 0$)

& $$\alpha = \alpha_1+\alpha_2, \quad \beta = \beta_1+\beta_2, \quad \gamma = \gamma_1+\gamma_2.$$

It follows that $$U_1 = \tfrac{1}{3}\ (A+B)\ (\alpha+\beta+\gamma)^2$$

$$U_2 = \frac{2A-B}{3}\ (\alpha^2+\beta^2+\gamma^2-(\beta\gamma+\gamma\alpha+\alpha\beta)).$$

Now my *opinion* is, that these two parts may be considered as independent U_1 being the work done in condensation and U_2 that done in distortion. Now I would use the old word "Resilience" to denote the work necessary to be done on a body to overcome its elastic forces.

The cubical resilience R, is a measure of the work necessary to be expended in compression in order to increase the density permanently. This *must* increase rapidly as the body is condensed, whether it be wood or lead or iron.

The resilience of rigidity R_2 (which is the converse of plasticity) is the work required to be expended in pure distortion in order to produce a permanent change of form in the element. I have strong reasons for believing that when

$$\alpha^2+\beta^2+\gamma^2-\beta\gamma-\gamma\alpha-\alpha\beta$$

reaches a certain limit $=R_2$ then the element will begin to give way. If the body be tough the disfigurement will go on till this function U_2 (which truly represents the work which the element *would do* in recovering its form) has diminished to R by an alteration of the *permanent dimensions*.

Now let $a\ b\ c$ be the *very small* permanent alterations due to the fact that $U_2>R_2$ for an instant. Whenever $U_2=R_2$ the element has as much work done to it as it can bear. Any more work done to the element will be consumed in permanent alterations.

Therefore if $U_2=R_2$, and in the next instant, U be increased, dU must be lost in some way.

My rough notion on this subject is that

$$a=\frac{dU}{U}\alpha,\quad b=\frac{dU}{U}\beta,\quad c=\frac{dU}{U}\gamma$$

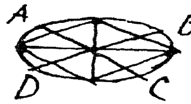
the new values of $\alpha\,\beta\,\gamma$ will be

$$\alpha'=\alpha-a,\quad \beta'=\beta-b,\quad \gamma'=\gamma-c.$$

This is the first time that I have put pen to paper on this subject. I have never seen any investigation of the question, "Given the mechanical strain in 3 directions on an element, when will it give way?" I think this notion will bear working out into a mathemat. theory of plasticity when I have time; to be compared with experiment when I know the right experiments to make.

Condition of not yielding

$$\alpha^2+\beta^2+\gamma^2-\beta\gamma-\gamma\alpha-\alpha\beta<R_2.$$



I have not had time to read your magnecrystalline spheres, but I shall tomorrow.

I have been proving the "law of areas" experimentally in the case of an ellipse performed by a funnel of sand hanging by a string. I draw conjugate diameters AC BD and collect the sand from AD & BC into one scale of a balance and that from AB & CD in the other and show that they very nearly balance when all goes well. The drawing conj. diam^r is the chief difficulty when the ellipse is like this, owing to precession of apses.

Where is Helmholtz on the Eye to be found?

I have continued a very adjustable pseudoscope wh: I am getting made by degrees.

Yours

J. C. Maxwell

8 Palace Garden Terrace, Kensington, W.
1861 *Dec.* 10

Dear Thomson

I have not heard of you for some time except through Balfour Stewart who told me he had seen you lately. I hope you are now well as you are at work.

I was not farther north than Galloway last summer and we spent all our three months vacation there. Since I saw you I have been trying to develope the dynamical theory of magnetism as an affection of the whole magnetic field according to the views stated by you in the Royal Society's proceedings 1856 or *Phil. Mag.* 1857 vol. I p. 199 and elsewhere.

I suppose that the "magnetic medium" is divided into small portions or cells, the divisions or cell-walls being composed of a single stratum of spherical particles these particles being "electricity". The substance of the cells I suppose to be highly elastic both with respect to compression and distortion and I suppose the connexion between the cells and the particles in the cell walls to be such that there is perfect rolling without slipping between them and that they act on each other tangentially.

I then find that if the cells are set in rotation, the medium exerts a stress equivalent to a hydrostatic pressure combined with a longitudinal tension along the lines of axes of rotation.

If there be two similar systems, the first a system of magnets, electric currents and bodies capable of magnetic induction, and the second composed of cells and cell walls, the density of the cells everywhere proportional to the capacity for magnetic induction at the corresponding point of the other, and the magnitude and direction of rotation of the cells proportional to the magnetic force, then—

1. All the mechanical magnetic forces in the one system will be proportional to forces in the other arising from centrifugal force.

2. All the electric currents in the one system will be proportional to currents of the particles forming the cell walls in the other.

3. All the electromotive forces in the one system, whether arising from changes of position of magnets or currents or from motions of conductors or from changes of intensity of magnets or currents will be proportional to forces urging the particles of the cell walls arising from the tangential action of the rotating cells when their velocity is increasing or diminishing.

4. If in a non conducting body the mutual pressure of the particles of the cell walls (which corresponds to electric tension) diminishes in given direction, the particles will be urged in that direction by their mutual pressure but will be restrained by their connexion with the substance of the cells. They will therefore produce strain in the cells till the elasticity called forth balances the tendency of the particles to move.

Thus there will be a displacement of particles proportional to the electro-motive force, and when this force is removed the particles will recover from displacement.

I have calculated the relation between the force and the displacement on the supposition that the cells are spherical and that their cubic and linear elasticities are connected as in a "perfect" solid. I have found from this the attraction between two bodies having given quantities of free electricity on their surfaces, and then by comparison with Weber's value of the statical measure of a unit of electrical current I have deduced the relation between the elasticity and density of the cells. The velocity of transverse undulations follows from this directly and is equal to 193088 miles per second, very nearly that of light.

Velocity of light miles per sec.	192,500 by aberration 195,777 by Fizeau 193,118 Galbraith & Haughton's statement of Fizeau's results

I made out the equations in the country before I had any suspicion of the nearness between the two values of the velocity of propagation of magnetic effects and that of light, so that I think I have reason to believe that the magnetic and luminiferous media are identical and that Weber's number is really, as it appears to be, one half the velocity of light in millimetres per second.

If there is in all media, in spite of the disturbing influence of gross matter, the same relation between the velocity of light and the statical action of electricity, then the "dielectric capacity", that is, the capacity of a Leyden jar of given thickness formed of it, is proportional to the square of the index of refraction.*

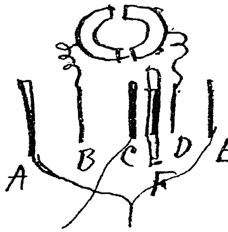
Do you know any good measures of dielectric capacity of transparent substances? I have read Faraday & Harris on the subject and I think they are likely to be generally too small. I think Fleeming Jenkin has found that of gutta percha caoutchouc &c. Where can one find his method, and what method do you recommend?

* These developments were published by Maxwell in *Phil. Mag.* 1861–2. Weber's ratio was *half* the speed of light on account of his special units. Soon after the law of attraction between moving Weberian electrons appeared, G. Kirchhoff (*Pogg. Ann.* 1847) developed the theory of their oscillations, and the resulting wave forms in a cylindrical conductor, finding as he remarked that provided the waves are short enough they travel along the surface of the cylinder with the speed of light.

As to Maxwell's prediction connecting index of refraction with square root of dielectric capacity, there is a most striking record in a letter from Boltzmann that is published in Königsberger's *Life of Helmholtz*. It appears that Boltzmann's earliest investigation long after (in 1872), in the laboratory of Helmholtz, was to test such a relation for the gases. But he thought that Maxwell required the index of refraction to be proportional to the dielectric capacity, and finding no such relation between his measures, dropped the subject. However, after he left Berlin, he happened to take up his records again, and noticed that they showed good agreement between the index and the *square root* of the capacity. On looking up Maxwell's Memoir he found that this was just what his theory required, and so reported, to his great delight, to his master Helmholtz. This confirmation was not in time for Maxwell's *Treatise* (1873).

I think I see a way to work with flat plates of different substances along with your divided ring electrometer.

A & *E* are plates connected with each other, and with a source of electricity; *C* is connected with the ground, *B* & *D* are moveable plates connected with the two halves of the ring. *F* is a dielectric. Then by a proper placing of *D* things may be arranged so that the electrification of *A* and *E* produces no difference of potentials in the divided ring, after which the capacity of *F* follows by calculation.



I have also examined the propagation of light through a medium containing vortices and I find that the *only* effect is the rotation of the plane of polarization in the *same* direction as the angular momentum of all the vortices, the rotation being proportional to

A the thickness of the medium
B the magnetic intensity along the axis
C the index of refraction in the medium
D inversely as the square of the wave length in air
F directly as the radius of the vortices
G ,, the magnetic capacity.

I have been seeking for experiments lately made which I have lost sight of showing that the rotation varies faster than the inverse square of wave length in air, so that *C* & *D* give the true law.

A & *B* are proved by Verdet,
F is not yet capable of proof

G is consistent with Verdet's result that the rotation in salts of iron is opposite to that in diamagnetic substances. I think that molecules *of iron* are set in motion by the cells and revolve the opposite way so as to produce a very great energy of rotation but an angular momentum in the opposite direction to that of the vortices.

I find that unless the diameter of the vortices is sensible, no result is likely to be obtained by making a magnet revolve freely about an axis perp. to the magnetic axis. I have tried it but have not yet got rid of the effects of terrestrial magnetism which are very strong on a powerful electromagnet which I use. I think it more probable that a coil of conducting wire might be found to have a slight shock tending to turn it about its axis when the electricity is let on or cut off*, or that a piece of iron magnetized by a helix might have a similar impulse.

Yours truly

J. C. Maxwell

* This subject of the mechanical reaction of magnetization is now much developed, resting on experiments of Einstein and de Haas, Bennett, Sucksmith, and many others.

8 PALACE GARDEN TERRACE, KENSINGTON, LONDON, W.
1861 *Dec.* 17

DEAR THOMSON

I have thought of an elementary investigation of the attraction of magnetic poles in a para or dia magnetic substance by means of the theory of electrical images applied to a simple case.

$A \quad \mu \quad B$

First. Let μ & μ' be the inductive capacities of two media bounded by an infinite plane.

$A' \quad \mu' \quad B'$

Let A be a magnetic pole, then the force and potential at any point B in the *same* medium are those due to A and its image A' provided $A' = A\,\dfrac{\mu-\mu'}{\mu+\mu'}$.

Also at any point B' in the other medium the potential is that due to $A\,\dfrac{2\mu}{\mu+\mu'}$, placed at A.

For we shall find that these values give the potential the same in both media at the bounding surface, and the variation of potential normal to the surface inversely proportional to μ and μ'.

Second. Let A and B be two magnetic poles distant x from each other and $\frac{1}{2}y$ from the surface and let the law of force be repulsion $= \dfrac{mAB}{x^2}$ where m depends on the medium.

$A \quad \mu \quad B$

$*A \qquad *B'$

The potential of B (with respect to A & the surface) is

$$mB\left\{\frac{A}{x}+\frac{\mu-\mu'}{\mu+\mu'}\left(\frac{B}{y}+\frac{A}{\sqrt{x^2+y^2}}\right)\right\}.$$

Hence if A and B are originally close to the surface at an infinite distance and are then carried by any paths in the first medium so as to be close to the surface and distant a from each other, the work done will be

$$mB\left\{\frac{A}{a}+\frac{\mu-\mu'}{\mu+\mu'}\,\frac{A}{a}\right\}=\frac{2\mu}{\mu+\mu'}\,m\,\frac{AB}{a}.$$

Now if the same thing be done on the other side of the bounding surface we must simply put m' for m, μ' for μ & μ for μ' and we find the work

$$=\frac{2\mu'}{\mu'+\mu}\,m'\,\frac{AB}{a}.$$

These quantities must be equal, otherwise we might do work by separating

two poles putting them into a medium and bringing them near and then taking them out and bringing them to their former position: whence

$$m\mu = m'\mu'$$

or the force is inversely as μ that is greater in dia- than para-magnetics.

This kind of argument does not directly apply to currents because they are doing work even when at rest, but it can be got at by considering

1st The potential of two currents is the one current multiplied by the number of lines of magnetic force *due to the other* which pass through it.

2nd the force on a magnetic pole at given distance from the current depends on the current and is independent of the medium. For the work done on a unit pole by one circuit round the current is $4\pi I$ where I is the current and if it depended on the medium work might be done by a pole going round a closed curve not embracing the current.

3rd The *number* of lines of force in any place depends on the magnetic force multiplied by the magnetic coefficient.

Therefore if the same currents are placed in two different media, the magnetic forces will be the same at corresponding points, but the potential of the currents will be *directly* as μ, and so will the forces and the inductive capacities of the wires if the currents be variable.

It is remarkable that the spontaneous movements of magnets tend to diminish their potentials, but the spontaneous movements of currents tend to increase their potentials and the whole energetic value of the field they move in.

Could it be proved that in any piece of machinery containing wheels mounted on moveable axes, whose motion alters their velocities:—If the machine be set agoing and left the axes will tend to move so as to diminish the actual energy of the machine. But if the motive power be continually applied to keep up a constant velocity in one wheel, the axles or moveable parts will move so as to increase the actual energy.

I am not sure of the truth of this in general with regard to the last part*, but here is an instance.

If a tube be set a spinning about an axis perp. to it and a bullet tied by a string to the axis be allowed to yield to centrifugal force by the elasticity of the string, the tube and ball together will lose *vis viva*, which will be stored in the elastic string.

But if the tube be kept spinning at constant velocity, the yielding of the string to centrifugal force will cause the tube and ball to have greater *vis viva*.

If this is a general truth it applies to magnets and currents, magnets not having any motive power to back them up like that which currents find in the battery.

* In generalization of Thomson's theorem of maximal energy, in Thomson & Tait's *Natural Philosophy* (1867).

I have not been able to ascertain whether any investigations of dielectric capacity have been made besides those of Faraday & Snow Harris and those on different kinds of gutta percha by Jenkin & others.

I think by your electrometer one might eliminate the effects of slow conduction, "penetration of charge" &c by charging the extreme or middle plates of your instrument instantaneously and then discharging and observing the impulse given to the needle.

Is your instrument such that when the needle is placed at zero the potential of the two halves of the ring may be suddenly changed without disturbing the needle provided the two halves have always the same potential?

I should like to hear from you some time whether you are likely to make any observations on dielectrics, and if you have not the prospect of doing so I should like to have the benefit of the plans you have proposed and to know which of your forms of electrometer you consider most suited for the purpose; for I have more time now to work, and I have several experiments in electricity which I wish to try, and I hope to get the dielectric coefficients of bodies relative to air as near the truth as I can.

I also, if I can get a good nonconducting liquid, mean to compare the force of two bodies in the liquid and out. If their potentials are given the force [expressed in terms of the potentials?] ought to vary directly as the dielectric capacity (greater in glass than in air). If their charges are given the force [expressed in terms of the charges?] ought to vary inversely as ditto. It would be difficult to plunge a ball in a liquid without altering its charge but to keep up its potential is easy. I also wish to ascertain whether this property has axes. If it has a sphere of crystal will *set* between two electrified plates.

I shall be glad to know the max$^{\text{m}}$ breadth of atoms. I suppose their length is not so easily limited, but if you could get a minimum breadth, you would go far to establish the existence of the atom.

I know experiments which may determine the maximum breadth of a vortex of magnetism [cf. p. 6] and if we could find the actual breadth we might find the density of ether and the time of revolution of a vortex. This is by a comparison of the rotation of pol. light by a known mag. force with the axis of free rotation of a magnet. I want to put Verdet's results into absolute measure, but I need the deflexion produced by a turn of *his bobbin* due to terrestrial mag. at Paris, which he has not given so far as I know. I think Arndtsen of Christiania works now at this.

Yours truly

J. C. MAXWELL

8 Palace Gardens Terrace, W.
1863 *May* 29

Dear Thomson

Can you dine with us on Tuesday 9th June?

[Printed here in illustration of the mode of joint experiment of Maxwell, Jenkin and Balfour Stewart on ratio of the static and kinetic units of charge. Cf. *Treatise*, § 779.]

On Wednesday 27th we made three determinations which I have now reduced. I give you the details of the 1st that you may see our present method.

(A) Determination of zero. Coil rotating + no current. 40 observations of extremities of oscillations

Mean of 1st ten (millimeters)	949·56
2nd —	950·55
3rd —	950·73
4th —	950·52
Mean	950·34

(B) Connexion made. Coil rotating + : and 80 oscillations taken

Mean reading of 1st ten			1237·38
		2nd	8·33
		&c	9·65
Time of 3000 revolutions			9·86
0 to	3000	530·0	8·25
300	3300	530·0	8·40
600	3600	530·0	9·35
900	3900	530·3	7·80
		Mean scale reading.	1238·6275

Time of 100 rev. $T = 17{\cdot}67$

C. Circuit broken rotation + zero reading again

	951·30
	50·87
Mean	50·00
After exp.	950·723
Mean zero before and after	950·504
Mean reading due to current	1238·627
deflexion $= \delta$.	288·123
Time of 100 revolutions $= T$.	17·67
Product $T.\,\delta. =$	5091·13

The second exp. went wrong in the spinning.

The third experiment

zero	957·00
negative rotation scale reading	668·936
deflexion negative	288·064
Time of 100 revolutions	17·6284
Product $T\delta$	5078·11

Fourth Exp.

rotation + Scale	1247·11
zero	957·00
deflexion	290·11
Time of 100 rev$^{\text{ns}}$	17·57
Product $T\delta$	5097·1

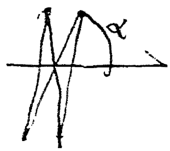
Jenkin has the coils which he equated to the spinning coil between these exp$^{\text{s}}$.

There was a change of zero which we must try to avoid, but I know when it took place.

Now for the measure of resistance

length of wire	303·403 meters
number of turns	308

Distance of scale from mirror 3000 millimeters (equal to a certain wooden rod which I shall measure again)



α = angle subtended by radius of coil at magnet 83° 11′

$$\sin^3\alpha = \cdot 978912.$$

Corrections for thickness &c of coil ·9996116

$$R \; \frac{200\pi^2 \times 3000 \times 303{\cdot}403 \times 308 \times 978912 \times \cdot 9996116}{T\delta}$$

+ correction.

Correction for induction of magnet at centre	+ ·00720
for torsion of fibre	− ·00135
Proportional correction	+ ·00585.

R.

Correction for reduction of tan 2ϕ to tan $\phi = 1{\cdot}000 \dfrac{\delta^2}{4D^2}$

for induction of coil on itself $= -\cdot 7331 \dfrac{}{4D^2}$

total $0{\cdot}2669 . \dfrac{\delta^2}{4D^2}$

Final formula for R

$$R = \frac{544648{,}000{,}000}{\delta T} + 4037\,\frac{\delta}{T}.$$

I have not divided out as I have no time.

Here are former values of $T\delta$ on a colder day

5176

5105

Jenkin cannot come on Wednesday but I think Thursday will do for an experiment as Stewart, Jenkin & I can repeat what we have done, and if you are engaged Thursday it does not so much matter as you could do us more good by advice when we are not spinning than when we are spinning. If you can come here on the 9th I will ask Jenkin & Stewart.

Yours truly

J. C. Maxwell

A. Cornu has set up a divided ring electrometer wh. he keeps charged with a dry pile. [W. T.]

Glenlair, Dalbeattie.
May 12, 1869

Dear Thomson

I am finishing what I have to say on Statical Electricity [for the *Treatise*? of 1873, cf. chapters x, xi] & I should be glad to hear from you on some points.

1 I think I am right in saying that Poisson, Plana &c though they determined the distribution on a sphere due to an electrified point did not discover that the potential due to this distribution on the outside is that due to the image of the point; and that this simplification of the theory, together with the whole theory of electrical inversion, belongs to you.

2nd The other problems solved by a general method (analogous to the harmonics) are

α Ellipsoids of revolution (planetary & ovary) by J. Neumann, *Crelle* 37.

β Indefinite right cylinder Kirchhoff, *Crelle* 48.

Has Liouville or any one else done it for the ellipsoid of 3 axes, except in the case of the charged ellipsoid in infinite field and the uncharged ellipsoid in field of uniform force?

In all the cases that have been done the position of a point in space is determined by three functions $\rho\,\mu\nu$. The equation of the conducting surface is

$$\rho = a.$$

For all internal points ρ is between 0 and a. For all external points ρ is between a and b which may be ∞.

The values of the potential are of the form

$$RMN$$

where R, M, N are functions of $\rho\,\mu\nu$ respectively.

For each value of M & N there are two values of R which fulfil Laplaces eq^{n} one which vanishes at ∞ and another which does not become infinite within the surface. M & N must be of the nature of periodic functions. See Lamé on Inverse Functions.

The case of 2 dimensions has been pretty well solved by C. Neumann, *Crelle* 1861.

Have you published anything about the theory of electrical images in the figure formed by four spheres which cut at right angles? [as in *Treatise* § 169].

For electrification to potential unity, if the radii are $r_1\, r_2\, r_3\, r_4$ the images are:

At centre of 1st sphere, charge $= r_1$ (four points).

At foot of perp from 3rd centre on the line between 1 & 2 charge $= -\dfrac{1}{\sqrt{\dfrac{1}{r_1^2}+\dfrac{1}{r_2^2}}}$

(six points of this kind).

At foot of perp from 4th centre on plane of 1 2 & 3 charge $= +\dfrac{1}{\sqrt{\dfrac{1}{r_1^2}+\dfrac{1}{r_2^2}+\dfrac{1}{r_3^2}}}$

(four such points).

At intersection of these four perpendiculars charge $= -\dfrac{1}{\sqrt{\dfrac{1}{r_1^2}+\dfrac{1}{r_2^2}+\dfrac{1}{r_3^2}+\dfrac{1}{r_4^2}}}$

(one point).

The surface for which the potential due to these 15 points is unity consists of the 4 spheres. Of course the case of this figure influenced by a point comes out by inversion.

Three spheres give the same expressions and so do two.

If there are two spheres, centres A, B, radii α and β and a point O distant a from A & b from B the density at a point P on the sphere A induced by unit of electricity at O is

$$\sigma = \frac{1}{4\pi}\,\frac{a^2-\alpha^2}{\alpha r^3}\left(1-\frac{\beta^3 r^3}{(\beta^2 r^2+(b^2-\beta^2)\,(p^2-\beta^2))^{\frac{3}{2}}}\right)$$

radii of spheres α & β [and they are in contact so that] distance $AB=\sqrt{\alpha^2+\beta^2}$

$$OA=a,\quad OB=b,\quad OP=r,\quad BP=p.$$

I have been doing the charge on a proof plane in the form of a disk of radius a and thickness b [as in *Treatise* § 196]. I find that the charge is greater than that of the circle it covers in the proportion of

$$1+8\,\frac{b}{a}\log\frac{8\pi a}{b}\quad \text{to}\quad 1.$$

The thickness is supposed small compared with the radius, and the radius small compared with the radius of curvature of the electrified surface to which the disk is applied.

I am now describing your absolute electrometer & the quadrant d^{o}. To what *class* do you refer Coulomb's Torsion Balance?

Where are you to be found this summer? I may be at Ardhallow, Dunoon, between May 17 and May 31, after that at Glenlair. How is the reprint of your electrical papers getting on?

Yours truly
J. CLERK MAXWELL

GLENLAIR, DALBEATTIE.
June 5, 1869

DEAR THOMSON

I am anxious to know whether your views about contact electricity are those which I attribute to you.

In the first place what is the potential of a metallic conductor? Is this it? Make a hollow place in the conductor and place the end of the electrode of an electrometer in the hollow space, and carry off a great number of small particles from the end of the electrode to an infinite distance, then the potential indicated by the electrometer differs from that of the conductor by a constant quantity.

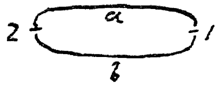
2nd The electromotive force from one metal to another is $J\Pi$ where Π is the Peltier effect as defined in your RSE paper. Now Π is a function of the temperature and of the 2 metals in contact and is such that at any one temperature for metals $a\,b\,c$

$$\Pi_{bc}+\Pi_{ab}+\Pi_{ca}=0.$$

Hence $\qquad \Pi_{ab}=P_a-P_b, \quad \Pi_{bc}=P_b-P_c, \quad \Pi_{ca}=P_c-P_a$

where $P_a\ P_b\ P_c$ are functions of the temperature for each metal.

Also in any one metal owing to difference of temperature the difference of potentials of any two points is equal to the difference of the values of a function Q of the temperature, so that for a circuit of two metals a and b of which the junctions are at temperatures 1 and 2 the total electromotive force is



$$F=P_{a_1}-P_{b_1}+Q_{b_1}-Q_{b_2}+P_{b_2}-P_{a_2}+Q_{a_2}-Q_{a_1}$$
$$=P_{a_1}-Q_{a_1}-(P_{b_1}-Q_{b_1})-(P_{a_2}-Q_{a_2})+(P_{b_2}-Q_{b_2}).$$

Hence if we make $P_{a_1}-Q_{a_1}=R_{a_1}$ and so on

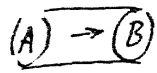
$$F=R_{a_1}-R_{a_2}-R_{b_1}+R_{b_2}.$$

If every P was equal to the corresponding Q thermo-electric currents would be impossible, but the Peltier effect at a junction might still exist, so that by thermal experiments we might discover both the Ps and Qs in the way that you discovered the Qs.

Could the Qs be measured by finding the difference of potential of air near hot & cold parts of the same piece of metal?

Here is my theory of the state of a dielectric called electric polarization.

Draw any closed curve and through the points of this curve draw lines of inductive action so forming a tube of inductive action. Draw two equipotential surfaces cutting the tube and enclosing between them a cell.



The resultant electromotive force in the small cell is $\dfrac{A-B}{AB}$ in the direction perpendicular to A or B and from A towards B. The electric displacement is a linear function of the electromotive force and is parallel to the surface of the tube. In fluid dielectrics the displacement is in the direction of the electromotive force and is proportional to it.

The displacement through any section of the tube is the same and is

$$\frac{A-B}{AB}\,D\,(\text{area})=Q$$

where D is the specific inductive capacity.

Besides the internal displacement, which consists in a quantity of electricity Q being forced through every section from A towards B we must suppose a superficial distribution of Q on section A and $-Q$ on section B. When the polarization is solenoidal these superficial electrifications destroy one another except at the surface of the dielectric.

The energy of the cell is $\frac{1}{2}FQ$ and its mechanical stress is a tension $=\frac{1}{2}\dfrac{FQ}{\text{volume}}$ on the surfaces A and B and a pressure of equal numerical value on the sides of the tube.

It follows from this theory that the movements of electricity are like those of an incompressible fluid, and that charging a body and putting it into a cubic foot of air does not increase the quantity of electricity in the cubic foot.

Yours truly

J. C. Maxwell

Ardhallow, Dunoon.
1 *October* 1869

Dear Thomson

Many thanks for yours of the 22nd Sept. You see I am again on the Clyde & I propose to be in Glasgow on Tuesday 5th Oct and will call at White's about 12 or as I hear from you.

In which of your papers is there a discussion of Poisson's k? Thalén gives for Neumann's k 32·32, 31·80, 32·61.

I have written 40 pages on Magnetic Measurements and am beginning

Terrestrial Magnetism. I have put in Joule's method of swinging two magnets together, so as to eliminate induction. I think that the magnets ought to be as heavy as a single silk fibre will safely carry and as short as is consistent with strong permanent magnetization.

I got into a mess with an investigation of the 2 best distances at which to place the deflecting magnet in order to deduce its moment by the eq^{n}

$$\frac{2M}{H} = \frac{D_1 r_1^5 - D_2 r_2^5}{r_1^2 - r_2^2}$$

supposing the relation between the probable error in the measurement of D the tangent of deflexion and r the distance to be known. In fact I got a pair of impossible values for r_1 & r_2. If one distance only is used the best distance is when

$$\frac{\delta D}{D} = \sqrt{3}\,\frac{\delta r}{r}$$

where $\frac{\delta D}{D}$ is the probable percentage error of D and $\frac{\delta r}{r}$ that of r.

Your bowl investigations are first rate. I must find the induction through a round hole in a plate by means of them. Whether would you have me bag the whole thing for my book, or give results and references with an *account* of the method?

Yours truly

J. Clerk Maxwell

C. Neumann's theory of the transmission of Potentials is altogether unique, the Potential $\frac{mm_1}{r}$ (not the potential function $\frac{m}{r}$) starts from m (with the consciousness of the value of r and m_1 at the instant) and travels along r with uniform velocity not absolute but relative to m till it reaches m_1 which receives it after a time t. Truly those who supposed that Neumann's potential travelled like light were greatly mistaken.

Ardhallow, Dunoon.
5 *Oct.* 1869

Dear Thomson

I did not go to Glasgow today as you were not to be there. I can go on Thursday Friday or Saturday. We are going home the beginning of next week. There are but few other things I have to do in Glasgow.

In Shurrocks' hairbrushing room I examined the endless band in motion. The velocity of propagation of a disturbance in space is in the case of a flexible

tube with water going at velocity V, when m = mass of tube μ of water in unit of length,

$$\text{vel of disturbance} = \frac{\mu}{m+\mu} V \pm \sqrt{\frac{T}{m+\mu} - \frac{m\mu V^2}{m+\mu}}.$$

When the 2nd term is impossible, that is if T the effective tension (tension-pressure) is small compared with V the motion is unstable. When however $m=0$ as at Shurrocks' this cannot be.

When the shape of the tube is in equilibrium $T = T_0 + \mu V^2$ where T_0 is the statical value. Hence the velocity may be written

$$\frac{\mu}{m+\mu} V \pm \sqrt{\frac{T_0}{m+\mu} + \frac{\mu^2 V^2}{(m+\mu)^2}}$$

which shows that the velocities are always in opposite directions but when T_0 is small one of them is very small. Hence the sluggishness of the wave near the bottom of a loop of chain or flexible pipe, part of it darts up the ascending side and part slowly ascends the descending one but faster as it gets away from the bottom.

Can you tell me where you were getting *light* indiarubber cloaks when I was at Largs? last year.

Yours truly

J. Clerk Maxwell

Glenlair, Dalbeattie.
16 *Nov.* 1869

Dear Thomson

Mr Tatlock tells me you want to know about the conductivity of liquids for heat. The last thing on the subject is Prof. F. Guthrie On the Thermal Resistance of Liquids (Proc. RS Jan. 21, 1869). He states in his paper (I do not know if it is printed or to be printed) previous results. His experimental methods seem very good. His chief defect is that he never seems to know what he is going to measure. He works at the Royal Institution and has been so impregnated with radiant heat and otherwise Tyndallized that he describes the specific resistance of a liquid to be the ratio of the quantity of heat *arrested* by the liquid to that arrested by an equal thickness of water.

He states his object to be "to determine the laws according to which heat travels by conduction through liquids" but he goes to work as if he wanted to find their absorption of radiant heat. The actual phenomena he observes are mainly phenomena of conduction.

He finds that heat gets through a millimeter of water in a minute very much better than any other liquid, their resistances being

Water	1	All solutions
Glycerine	3·84	of salts
Acetic acid glacial	8·38	increase
Alcohol	9·08	resistance
Oil of Turpentine	11·75	or diminish
Chloroform	12·10	conductivity
		&c

Of course mercury is not in this series. From my recollection of the paper the only results previously obtained were proofs that there is such a thing as true conduction of heat by liquids.

Perhaps Dr Matthiessen could give you information. He knows what conduction means and he collects the properties of bodies, and he will not tell you what he does not know.

For experiments on steady conduction an iron plate covered with copper on both sides might be useful as a measurer of the flow of heat. The two copper plates to be connected with a galvanometer. The galvanometer having a great resistance compared with the plate, its indications may be trusted to give the mean electromotive force over the plate or the mean flow of heat per square inch. The value of the galvanometer readings must be found by regular calorimetric methods.

The compound plate being a much better conductor than most of the substances to be tried and also probably thinner, the differences of temperature in the other bodies may be found by more direct methods of thermometry.

With respect to stress in a medium arising from magnetism

$$\frac{d}{dx}p_{xx}+\frac{d}{dy}p_{yx}+\frac{d}{dz}p_{zx}$$

is the x-force on an element of the medium referred to unit of volume. If in that element there is neither electric current nor magnetization $X=0$. If there is electric current the right expression comes out. If there is magnetization the case is more difficult because of the double def$^{\rm n}$ of force within a magnet. If there is nothing but Ampère's currents and if these are recognized as currents then all is easy, and

$$X=\alpha\rho-\beta w+\gamma v$$

where $\alpha\beta\gamma$ are components of magnetic force uvw are components of currents $\rho=$ density of imaginary magnetic matter

$$4\pi u=\frac{d\gamma}{dy}-\frac{d\beta}{dz}\ \&\text{c}, \qquad \rho=\frac{d\alpha}{dx}+\frac{d\beta}{dy}+\frac{d\gamma}{dz}$$

Yours truly

J. CLERK MAXWELL

CAVENDISH LABORATORY.
7 *March* 1878

DEAR THOMSON

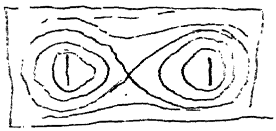
Stress in a gas of non-uniform temperature [in *Phil. Trans.* Memoir of same year 1878]

$$P = p - \frac{2}{3}\frac{R\theta}{C}\left(\frac{du}{dx} - \frac{1}{3}\left(\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz}\right)\right) + \frac{R^2\theta}{3C^2\rho}\left(\frac{d^2\theta}{dx^2} - \frac{1}{3}\left(\frac{d^2\theta}{dx^2} + \frac{d^2\theta}{dy^2} + \frac{d^2\theta}{dz^2}\right)\right)$$

Q & R similar $\qquad S = -\frac{R\theta}{3C}\left(\frac{dw}{dy} + \frac{dv}{dz}\right) + \frac{R^2\theta}{3C^2\rho}\frac{d^2\theta}{dz\,dy}$

p = hydrostatic pressure, $\frac{R\theta}{3C} = \mu$ = coeff^t of viscosity.

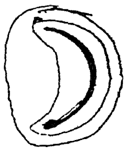
Hence $\frac{R^2\theta}{3C^2\rho} = \frac{3\mu^2}{\rho\theta}$ (N.B. The approximations assume that p is large compared with the terms in the last set. When ρ is zero they do not really become ∞. At a millionth of an atmosphere $\frac{\mu^2}{\rho} = 25 \cdot 9$ for air in CGS measure, so it is not small. Hence if there are two hot bodies in a cold vessel there will be a pressure between them because the $\frac{d^2\theta}{dx^2}$ between them is +. If they are colder than the vessel vice versa. Two infinite flat parallel plates would *not* repel each other whether hot, cold, or one hot & one cold. There would only be an effect near the edges [which has recently been formulated into a theory]. But the most remarkable thing is the equation of motion



$$\rho\frac{\partial u}{\partial t} + \frac{dp}{dx} - \mu\left(\frac{d^2u}{dx^2} + \frac{d^2u}{dy^2} + \frac{d^2u}{dz^2}\right) - \frac{1}{3}\mu\frac{d}{dx}\left(\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz}\right)$$
$$+ 2\frac{\mu^2}{\rho\theta}\frac{d}{dx}\left(\frac{d^2\theta}{dx^2} + \frac{d^2\theta}{dy^2} + \frac{d^2\theta}{dz^2}\right) = \rho X.$$

From the form of this equation it appears that the inequality of temperature will not produce any current (as indeed might be predicted if gravity were absent) but there will be a state of steady flow of heat without current, hydrostatic pressure uniform but yet stresses different in different directions.

Here then we have a kind of stress increasing as the density decreases (but not ∞ at zero) which does not produce currents but may act on solid bodies, and which is nonexistent when the temperature is a linear function of x, y, z. For similar systems it is inversely as the square of the dimensions. Hence small radiometers answer best. The isothermal surfaces for a hot cup are like the electric equipotentials of the same. Hence $\frac{d^2\theta}{dx^2}$ is greatest on the convex side and the cup is driven mouth foremost. [But see *Phil. Trans.*]



Yours very truly
J. CLERK MAXWELL

GLENLAIR, DALBEATTIE.
1879 *August* 25 [time of his final illness]

DEAR THOMSON

Professor Stokes has sent me again our reports on Osborne Reynolds, together with two long letters from O. R. in answer to the advice culled by Stokes from said reports and sent as suggestions to O. R.

The labour O. R. has had to go through in reperusing his own MS. seems to have impressed him with new views as to the trouble likely to be experienced by a Referee, and also by the Sec. R. S., who has to harmonize two referees and one author to the satisfaction of a committee of papers.

The author has also given up his method of determining the 3 components U, V, W, of mean velocity from the two quadric equations

$$\Sigma\,(\xi - U)^2 = \Sigma\,(\eta - V)^2 = \Sigma\,(\zeta - W)^2.$$

This was decidedly the most striking mathematical conception in the paper, so that when it is removed we shall be less likely to come into collision with him.

His reason for not quoting Stoney, viz that his dispute with [G. J.] Stoney is in the *Phil. Mag.*, can be settled by a foot note giving the reference. I certainly do not believe in the Stoney Stratum though the Crookes Layer is a thing capable of being explored.

If a hot plate discharged all molecules out of a broadside of guns normal to its surface it might produce a Stoney Stratum, but then it would require an army of demons to glean the molecules as they recoiled and aim them normal to the plate again. [Cf. modern emission of electrons.] This is the only way to maintain a uniform aeolotropic stress throughout a finite region of gas.

Otherwise the stress is uniform in all directions with a correction of the form

$$\frac{d^2\theta}{dh^2}\left(\text{not } \frac{d\theta}{dh}\right).$$

I never sailed through a plate of meerschaum on a molecule, so that I do not know whether the passage is like the Forth & Clyde canal or like the Caledonian canal but I should not expect to have many long reaches, but rather plenty of locks and sluices so that it would not be very like a tube but more like a cullender. Nevertheless I do not see how O. R. could detect all this between the lines of my paper of 1867.

My principal question is. What are your coordinates? and shall I send Stokes' bundle in that direction with any chance of its being reflected back with suitable reflexions to the Observatory, Armagh within a reasonable time?

As for me I was for 6 weeks or so in a state of Thermal Transpiration in Feb. & March, and if any more papers are sent me I shall be an exhausted receiver, for I am hardly able to correct proofs of Electricity & Magnetism [2 ed.].

When I saw you in Cambridge I did not know that W. Garnett, my demonstrator was going in for Edinburgh. He only made up his mind to do so at the last moment, just before the election. If he should go in for Glasgow I may say that his strength is as the strength of 10 because his heart is pure. Whatever he undertakes he does it with his might. He has out of his own energies developed a course of Nat. Phil for elementary men, in which the experiments are not only exhibited but are reasoned from, and the intellectual status of those who attended them was raised pretty considerably, so that the results contrasted favourably with those obtained by mathematical coaching.

I do not think he has done much in the Theory of Numbers and that sort of mathematics but he is up in all the practical part, and knows how to use it himself and how to inculcate it into others. He has also great power of grinding up a new subject for himself and boiling it down for his pupils. He quite sees the difficulties of things, but if they have to be done he always does them, and that within the specified time. In short he is one of the few men I know who has a strong will and knows how to govern it. He has just married and M^rs Maxwell's opinion of the lady is very favourable.

GLENLAIR, DALBEATTIE.
1879 *Sept.* 1

DEAR THOMSON!

Peacocks as Gardeners. We got our original stock from M^rs M^cCunn, Ardhallow. At that time (1860) the garden there was the finest on the coast and the peacocks sat on the parapets & banks near the house. After those days there came a gardener who found that the peacocks destroyed the garden, so the peacocks were destroyed, and the garden ceased to produce the commonest vegetables, and became quite a desert. M^r M^cCunn was very fond of his garden and very particular about it, but he also cared for his peacocks and whenever he went out he had bits of bread &c for them, by which he generated a centre of attraction

near the house and the peacocks were the great beauty of the place. When he died, of course Mrs McCunn let the gardener be master, so the peacocks died and the garden is nothing worth to see.

Mrs Maxwell says that if you were constantly at home and combined the feeding of peacocks with your morning smoke and meditation, the birds would soon be concentrated near the centre of attraction thus provided and would not trouble the garden. Mrs Maxwell always gets the peacocks to choose the gardener and they have chosen one who has now been seven years with us.

At seed time they are confined in an outhouse where they have some Indian corn and a vessel of water. When the hen is sitting she is not shut up, for she keeps to her nest and nobody is supposed to know where that is, but she comes once a day to the house, and calls for her dinner, and eats it, and goes back to her nest at once.

The peacocks will eat the young cabbages but the gardener tells them to go out again and they find it pleasanter to be about the house and to sit on either side of the front door.

Mrs Maxwell will not send them unless on consideration they would be acceptable.

I send O. R.'s reports and his letters. "Thermal Transpiration" would be called by Graham "Thermal Diffusion" or *even* "Effusion". I note some references to properties of bodies in recent publications.

Specific heat of gases. Eilhardt Wiedemann *Phil. Mag.* 1876.

Calorimetric data. A. Schuller in V. Wartha (Budapest).

Unit of heat $= \frac{1}{100}$ (gramme H_2O at 100° C.—do at 0° C).

Latent heat of water 79·86. Regnault 78·39.

Heat of combination of H_2 and O at 0° C.

Andrews	1845	33613	*Wiedemann's Annalen*
Than	1877	33867	Bd II s 357.
Thomsen	1872	33971	
Favre & Silb.	1845	34065	
Schuller & W.	1877	34126	

Diffusion calculated from Graham's experiments *Phil. Trans.* 1861 by Stefan, *Wiener Sitz.* 23 Jan 1879 $\frac{(\text{centimetre})^2}{\text{Day}}$

Caramel	10° C	$k = 0{\cdot}047$
Albumin	13° C	0·063
Cane sugar	9°	0·312
NaCl	5°	0·765
	9°	0·910
HCl	5°	1·742

Zinc Sulphate in water by H. F. Weber, *Züricher naturforschenden Ges* 25 Nov 1878

Percentage	Temp	k	$\frac{(\text{cm})^2}{\text{day}}$
0·3120	1°·20	0·1252	
—	18°·55	0·2421	
—	44°·70	0·4146	

$\{k = 0{\cdot}1187\,[1 + 0{\cdot}0557t]\}$

Stefan *Wiener Sitz.* March 1878

CO_2 in water at 16°·5 $k = 1{\cdot}41\,\frac{\text{cm}^2}{\text{day}}$

In alcohol at 16°·6

	CO_2	O	N
absorption	3·122	0·284	0·121
diffusion	2·772	1·243 × 2·66	

J. Schuhmeister *Wiener Sitz.* 3 April 1879.

Diffusion of salts increases with temperature also with concentration.

10 per cent solutions at 10° C $\frac{\text{cm}^2}{\text{day}}$

KCl	1·10	NHaCl	1·04		
KBr	1·13	NaBr	0·86	LiBr	0·80
KI	1·12	NaI	0·80	LiI	0·80
KNO_3	0·80	NaCl	0·84	LiCl	0·70
K_2CO_3	0·60	$NaNO_3$	0·60	CaCl	0·68
K_2SO_4	0·75	$NaCO_3$	0·40	$CuCl_2$	0·43
		Na_2SO_4	0·66	$CuSO_4$	0·21
				$ZnSO_4$	0·20
				$MgSO_4$	0·28

Baumgarten, *Wiener Sitz.* 8 Feb 187

diffusion of vapours in gases.

	H	Coal gas	Air	CO_2
temperatures	(16·8)	19·9	16·1	18·3
Schwefelälher	0·244	0·165	0·110	0·089
Schwefelkohlenstoff	17·3	19·0	17·9	17·3
	0·261	0·125	0·078	0·063
Chloroform	18·1	17·4	18·4	17·9
	0·270	0·125	0·074	0·047
Alkohol	17·9	16·5	17·1	
	0·344	0·173	0·096	

Your objection to the phrase "centre of gravity" of bodies in general is thus expressed by Lord Mahon (afterwards Stanhope of the Printing Press, a pupil

of Le Sage) Principles of Electricity 1779 "So that mathematically & correctly speaking the *common Definition* of the *Center of Gravity* of a Body (viz that Point, upon which, if the Body be suspended, in *any* position whatever it will always remain in perfect *aequilibrio*) is a compleat absurdity, and no less than a contradiction in terms".

$$\frac{1}{\frac{dp}{dt}}*$$

[Postcard, undated, perhaps about 1869: but marked "Seen Sep. 22 '99. K"]

The Tomlinson Correspondence is found.

Our knowledge of magnetization is probably statistical. The doctrine that magnetic energy is potential ditto. while electromagnetic energy is kinetic leads to hopeless confusion especially when currents act on magnets. Now a current is certainly a kinetic thing Q.E.D. It is very remarkable that, in spite of the *curl* in the electromagnetic equations of all kinds, Faraday's twist of polarized light will not come out without what the schoolmen called local motion.

$$\frac{dp}{dt}$$

Excursus

THE CARNOT-KELVIN ASPECT OF THERMODYNAMICS

According to Carnot's postulate heat can give rise to work only by falling to lower temperature. This negation ensures that at each temperature a definite physical system possesses a work-function A, named by W. Thomson its available energy at that temperature. This aspect of Carnot's postulate was enforced especially in Thomson and Tait's *Nat. Phil.* (1867). These available energies at different temperatures combine into one more general function, the *isothermal available energy*, which is a function of *temperature* θ as well as configuration. (Cf. the Minkowski condensation of personal spaces and times into a single universal space-time.) Thus we are entitled to assert the equation

$$\delta A = -\Psi_1 \delta\psi_1 - \Psi_2 \delta\psi_2 - \ldots - \eta\delta\theta,$$

where Ψ_r is the force exerted by the coordinate ψ_r of configuration. The final term in δA though regular has not to do with ostensible work: and $-\eta$, as yet arbitrary, here equal to A/θ, may be described as the thermal capacity or specific heat of available energy.

This, the one fundamental equation of thermodynamics, transforms into

$$\delta\ (A+\eta\theta) = -\Psi_1 \delta\psi_1 - \Psi_2 \delta\psi_2 - \ldots + \theta\delta\eta.$$

* Of course you (oh [Lewis] Campbell [his biographer]) know this is Maxwell's favourite signature! [W.T.]

This form of it implies that when η is maintained constant, there is also an available energy function, now $A+\eta\theta$ or say A', but expressed in terms of the configuration and entropy.

As a special case this latter formulation A' covers the manifestations of total energy when no heat is allowed to enter or leave the system. For conservation of internal energy involves null heat supply, so that by considerations like the above $\theta\delta\eta$ must vanish when δH is null, therefore it must be $\lambda\delta H$, where δH is the heat that enters the system in the change expressed by the operator δ in this formula. The value of the multiplier λ is an affair of the unit of temperature and may be put equal to unity. We may now describe our inscrutable universal phenomenon temperature as the negative gradient of this *adiabatic available energy* A' with respect to η. This quantity A is often named free energy after Helmholtz, perhaps because it is unaffected by gain of heat from outside. For the definition of gain of heat will now be afforded by the equation $\delta H=\theta\delta\eta$. Clausius made this function η of configuration and temperature fundamental, and gave it the name entropy. It is the counterpart of temperature in an analytical quadratic theory. The basic theorem of Clausius is that the entropy of a self-contained physical system cannot diminish; from the present point of view it asserts that the value of A must trend downwards towards the coordinate plane of constant temperature as temperature increases, a criterion which offers abundant latitude.

The Maxwell-Willard Gibbs criteria of change of a physico-chemical system are that the trend of a system kept isothermal, i.e. with θ constant, is towards internal decrease of A, while of the system kept isolated, i.e. with η constant, it is towards decrease of A'. If the system is to be referred to changing internal stress, the modified functions obtained by subtracting $\Sigma\Psi\psi$ are to be substituted for A and A', and now express trend of change in terms of stress instead of configuration.

Different phases of a chemical system may coexist in equilibrium across an interface, as explored by Andrews, and reasoned about by Maxwell and Gibbs, culminating in one direction in Willard Gibbs' guiding rule prescribing a quite small limit for the number of the possibilities of such coexistent phases.*

What is the foundation of the Clausius theorem of increasing entropy? This involves a correlative inquiry, What is heat; is it even a definite entity? The scrutiny of this question required some special type of actual physical system to which it could be attached, by way of illustration. Clapeyron, the earliest expositor of Carnot, explored graphically the case of cyclic working with a perfect gas, whose very simple properties had been determined experimentally mainly by the French school of physical chemists; but by an error in the

* See the additions (1928) inserted in Vol. II of the writer's *Mathematical and Physical Papers* (guided by the Index).

elementary algebra he managed to conform to the prevalent postulate of caloric, which required that no aggregate of heat is lost when the gas does work in the manner of a cyclic engine. This apparent violation of conservation of total energy was easily put right by Clausius in a paper which has held a prominent place in the science: it is, however, only an elucidation, far from a universal proof, of the deep-seated cyclic presumption or postulate of Carnot. But the error had been put right ten years earlier in Waterston's path-breaking memoir on the theory of gases, as will appear from close attention to its desultory mode of development; for to it the scrutiny was of course relevant, and essential. It was, however, preserved unpublished, owing to this want of system, in the Archives of the Royal Society. What Thomson could not make out appears to have been why heat should differ from other kinds of mechanical energy; nor could Clausius have offered any explanation why. The explanation, or rather expanded consideration, came from Thomson himself, as above, by the introduction of the concept Available Energy (A) as distinct from Total Energy (A'). This involves that there is no such definite entity as total heat or thermal content in a gas, or any other physical system; heat becomes an exact concept only differentially through the entropy of the system by means of the equation of definition $\delta H = \theta\delta\eta$. The upshot of the unprovable basis postulate is that Kelvin's quantitative experimental confirmation of James Thomson's prediction of the lowering of the freezing point of water, whose structure is unknown, by pressure was really a decisive landmark in the science. Indeed the working of a thermal engine is usually in like manner an affair of interchange between two phases, such as steam and water, of the same substance. For a gas engine of the Carnot-Stirling type becomes reasonably reversible only in the limit when it is worked very slowly.

This concept of entropy is most readily illustrated from systems constituted molecularly, such as in the simplest case of a gas. But the slightest consideration of biological phenomena surely shows that molecular structure does not penetrate to the bottom of things, is only the surface generality that in the simpler range of occurrences is skimmed off into a physical science. The question remains, whether or how far the entropy of Clausius, which need not be confined to molecular structures, and so may go far deeper, is adequate for a universal formulation. In formal thermodynamics, for example after the manner of Gibbs, molecular structure need not be introduced, though Gibbs himself makes it the foundation in his later exposition of a science of "Statistical Mechanics". There, following up Boltzmann's idea, entropy arises as a summary measure, duly restricted to be of additive type, of the degree of derangement of the molecular or other imperfectly fathomed features of the discrete system that are involved.